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*Cover illustration:* A detail from Juan Bautista Maino's *Recapture of Bahia* (1633) inspired by Lope de Vega's play *El Brasil restituido*. While the armada rides at anchor, Don Fadrique de Toledo presents to the defeated Dutch an allegorical tapestry of Philip IV being crowned with laurels by the count-duke of Olivares. Beneath Philip's feet lie Heresy, Discord, and Treachery (the New Christians). In the foreground, the local Portuguese residents treat a wounded nobleman (a Castilian?). The figure immediately behind Toledo may be Manuel de Meneses, commander of the Portuguese forces. Courtesy of the Prado Museum, Madrid. See the article in this issue by Stuart B. Schwartz, "The Voyage of the Vassals: Royal Power, Noble Obligations, and Merchant Capital before the Portuguese Restoration of Independence, 1624–1640," pp. 735–67.



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# The American Historical Review

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## In This Issue

Peter Novick's book, *That Noble Dream*, received extended reviews last year in prominent journals, including a review essay in the *AHR* by James Kloppenberg (October 1989). These commentaries clearly did not exhaust discussion of Novick's fascinating survey of American historians' discourse on objectivity. A panel organized by **Dorothy Ross** for the most recent AHA Annual Meeting continued the debate and featured an unusually broad range of opinion on the book; **J. H. Hexter**, **Linda Gordon**, **David Hollinger**, and **Allan Megill** offered critiques, to which **Peter Novick** responded. This exchange, now an *AHR Forum*, leads off the June issue.

Research on the varied approaches to the abolition of slavery continues unabated after several decades of productive work. **Seymour Drescher** compares the second French abolition in 1848 with the British mode of abolition to which it aspired. He modifies the usual view of a peculiarly "French" revolutionary approach to abolition that was supposedly characteristic of both the First and Second Republics.

**Stuart B. Schwartz** examines the relations in Iberia between crown, nobility, clergy, and merchant interests, especially the role of Christianized Jews, through the prism of events surrounding the loss and recapture of Bahia (Brazil) to the Dutch. The social crisis that followed these events sounded the death knell of an integrated Iberian monarchy. Schwartz gives a fresh understanding of this story by showing the ability of the Portuguese Inquisition and nobility to thwart governmental efforts on behalf of mercantile interests, a conflict that led to the movement for Portugal's independence in 1640.

Just as West Europeans read their hopes and prejudices into the exotic worlds they conquered and Americans projected their fantasies and fears onto the Wild West, Russians nourished several visions of Siberia. **Mark Bassin** regards the various Russian ways of thinking about Siberia in the nineteenth century as keys to the outlook of educated Russian society. Interestingly, at one point, American thinking about the west, especially the fiction of James Fenimore Cooper, exerted a powerful influence on the Russians' view of Siberia and thus of themselves. Bassin's central purpose is to explore the characteristic European behavior of constructing geographic images that then become for their creators fundamental categories of their own self-conception and self-identification.

The emancipation of the Russian serfs in 1861 has been described as the greatest act of social engineering in European history before the twentieth century. Scholars have concentrated almost exclusively on the political and social aspects of this event and have failed to notice that the harsh terms of the emancipation for the peasants were dictated by a collapse of state credit institutions on the very eve of the reform. **Steven L. Hoch** traces the roots of this banking crisis and illuminates the confused policies that precipitated it. In the process, he gives us an altogether new picture of the emancipation statute and the problems of its subsequent implementation.

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## LATIN AMERICA



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**Peter Novick** is professor of history at the University of Chicago. He received his doctorate from Columbia University in European history but has since migrated to American history. The author of *The Resistance vs. Vichy: The Purge of Collaborators in Liberated France* (1969), and, most recently, *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (1988), which won the American Historical Association's Albert J. Beveridge Award for the best book of the year in American history, Novick is currently writing on the social and ideological uses of the Holocaust in the United States, for which project he received this year a fellowship from the National Endowment for the Humanities.

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*AHR Forum: Peter Novick's That Noble Dream:  
The Objectivity Question and the Future of the  
Historical Profession*

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The following essays are the products of critiques (and a reply by Peter Novick) delivered orally on December 28, 1990, at the Annual Meeting of the American Historical Association, New York. The panel was organized by Dorothy Ross, who initiated the session by introducing the participants and outlining their points of view. Here she appends an afterword, which summarizes the discussion and includes some comments made from the floor. The panel attracted a large crowd that filled the aisles, the floor space in back of the room, and spilled into the corridor outside—The Editor

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Carl Becker, Professor Novick, and Me; or,  
Cheer Up, Professor N.!

---

J. H. HEXTER

IN THE NEXT LITTLE WHILE, I propose to show that my hitherto-expressed views on Carl L. Becker (1873–1945) do better justice to him than do those of Professor Novick, hereafter Professor N. Further, I hope to show that this betterness has implications subversive to the theme of Professor N.'s book *That Noble Dream*. Its theme is that, from its beginning, the historical profession in the United States burned incense before the One True God of Objectivity. Between the World Wars, iconoclasts—the “new historians”—set about systematically desecrating the altar of that paltry god to the distress of the true believers. After the Second World War, however, the orthodox reasserted themselves, purified the temple of objectivity, and resumed their dismal rites. In the 1960s, however, the “ideological consensus” in support of objectivity “collapsed, . . . not to be reconstructed in subsequent decades.”<sup>1</sup> *That Noble Dream* ends in 1988 with an almost touching epitaph for the dead donkey of Objectivity.

In those days there was no king in Israel;  
every man did that which was right in his own eyes.<sup>2</sup>

<sup>1</sup> Peter Novick, *That Noble Dream: The “Objectivity Question” and the American Historical Profession* (New York, 1988), 415.

<sup>2</sup> Novick, *That Noble Dream*, 628, quoting Judges 21:25.

The book sets forth “the objectivity question,” as it worked itself out among Americanists, in the World-Historical Framework provided by the First World War, the Great Depression, the Second World War, the Cold War, the Vietnam War, and the Student Revolution of the later 1960s and the 1970s—big H History, indeed. Near the center of Professor N.’s big H History stood a quintessentially small h historian, Carl Becker.

In his presidential address to the American Historical Association in 1931, “Everyman His Own Historian,” Becker set off the avalanche that for a while buried the “objectivists.” Becker’s address showed that the goal of a—I quote Professor N.—“comprehensive, definitive objective reconstruction of the past was not just unattainable in the past, but a vacuous idea in principle.”<sup>3</sup> Two years after Becker, and from the same platform, Charles Beard preached a lay sermon on relativism that gave Professor N. the title for his book.<sup>4</sup> He thereby incited one of those professional donnybrooks for which he had an insatiable appetite. Yet his bully use of his bully pulpit was of less long-term effect in eroding the complacent overconfidence of the orthodox “scientific” historians than was the gentle penetrating action of Becker’s emollient discourse; at the end of Becker’s address, an applauding audience had risen to its feet. So far, so good.

Here, however, Professor N.’s big H History and my small h history part ways on how to read Carl Becker. Professor N., examining the *mentalité* of the historical profession in the United States in the context of the *mentalité* of the West, in the context of the *histoire totale* of the world for the past hundred years, thinks he is constrained to a panoramic vision. Therefore, he suggests, none of the hundreds and hundreds of people he shows in his historiographic Mardi Gras can be held by his camera long enough to allow the spectators to get a sense that they know whom they are looking at.

Professor N. was correct, however, in believing that, to get his enormous data mass in order, he needed at the outset a historiographical scaffolding. Without it, the whole enterprise becomes the sort of pack rat’s *omnium gatherum* that Becker found so silly when set out as “scientific” history by his pompous peers.<sup>5</sup> The scaffolding of Professor N.’s big H History starts him off with large questions, deep running tides, portentous rumbles. Gradually, the inquiries of *That Noble Dream* do pull closer to actual people; to several Big People at the top of the heap—Ranke, Marx, Freud, Sartre; to nobly heroic Charles Beard and less heroic Carl Becker; to the bottom of the heap, where one finds “objectivist,” ignoble me.

Big H History has both costs and risks. As Professor N. makes clear especially in explicit and scandalous footnotes, the record of the past, like Everyman’s memory and memos, is a lot of bits and pieces. Can small h history help us here? When a small h historian fixes his attention on a fragment of the past washed up on the littered beach of the present, he is likely to ask simple questions about it. What the devil is it? What was it for? Where is it from? How did it get here? What

<sup>3</sup> “Everyman His Own Historian,” in Carl Becker, *Everyman His Own Historian* (New York, 1935), 233–55; Novick, *That Noble Dream*, 254.

<sup>4</sup> Charles A. Beard, “That Noble Dream,” *AHR*, 41 (1935): 74–87.

<sup>5</sup> Carl Becker, “Detachment and the Writing of History,” *Atlantic Monthly*, 106 (1910): 524–36.



happened to it? Who in the world made this thing? What could he have had in mind? And how do I tell about this stuff?

Try to imagine a person who puts telling about that sort of thing—actually *writing* history—ahead of *being* a historian, Heraclitean doing ahead of Eleatic being. Well, on Carl Becker's word in 1941, his call to writing came some seven years *before* he considered *being* a historian as a vocation.<sup>6</sup> Can one give credence to the alleged recollections of a small h historian, sixty-eight years old, as he wrote? Actually, I happen to know an eighty-year-old small h historian who does. His own call to his vocation came to him at age five. He crawled into the center of the quiet kindergarten circle, put his head down, and kicked up his heels. Squeals of peer group approval! His debut as a public wise guy! Ten years later in H. L. Mencken, he found his earliest role model. Perhaps, however, to pursue his vocation of a public publishing wise guy, he might need to know a little about something, history perhaps—why not? I do know that, at eighty, that wise guy still recalls vividly, as Becker alleged he did at sixty-eight, the earliest intimation of his own vocation to write history. Indeed, he has just described the onset of those intimations.

We have reached the point where visual aids are more effective than verbal arabesques. It is time for show-and-tell. Consider first Exhibits *small a* and *small b* on the next page.

We can see at once that the exhibits are from a seedy, abandoned neighborhood; hardly anybody is around, just two old gaffers, one coming up on four centuries old, Pascal; the other, Archimedes, over two thousand years old. The language of both the exhibits, while not actually shabby, is not all that fancy, either. And, while both contain a bit of intellectual show-boating, neither is a truly high-tech verbal operation. Indeed, palpably low-class; compare with *BIG C*.

There it is, the right stuff, the real McCoy! How really real it is the type faces show.

*BIG C* is a crowded page, as densely peopled with **names** as *small a* and *small b* are desolate. There are lots of recent celebs around and lots of recent fancy intellectual chit-chat out on the cutting edge of thought, rather like a cocktail party on Central Park West, Manhattan, in the apartment of a Professor of Philosophy.

The capitals bold-faced people are crowded into the first paragraph of *C*. That is, into considerably less than half of the exhibit. An even heavier concentration of fancy suffixes (in lower-case bold-face)—**-ean, -ian, -ic, -ism, -ist, -ity, -logy, -ysis, -yst**, no less than nineteen, is mixed in with all those people.

In the second paragraph of *BIG C*, the author briefly appears to be recuperating from the alarming attack of -ysis he suffered in Paragraph 1, alas! only to succumb to an onslaught of bold-face lower-case "coherences" and "correspondences." So acute is this rare infection that the plague of polysyllaby is just about as lethal in the second paragraph as in the first.

Then suddenly the deadly chatter of people outshouting each other with overwrought abstractions stops at the word "disciplines" four lines from the end

<sup>6</sup> See Carl Becker, "The Art of Writing," *Detachment and the Writing of History* (Ithaca, N.Y., 1958), 122–24; and "Frederick Jackson Turner," in Becker, *Everyman*, 190–96.

*a*

We are apt to think of the past as dead, the future as nonexistent, the present alone as real; and prematurely wise or disillusioned counselors have urged us to burn always with "a hard, gemlike flame" in order to give "the highest quality to the moments as they pass, and simply for those moments' sake." This no doubt is what the glowworm does; but I think that man, who alone is properly aware that the present moment passes, can for that very reason make no good use of the present moment simply for its own sake. Strictly speaking, the present doesn't exist for us, or is at best no more than an infinitesimal point in time, gone before we can note it as present. Nevertheless, we must have a present; and so we create one by robbing the past, by holding on to the most recent events and pretending that they all belong to our immediate perceptions. If, for example, I raise my arm, the total event is a series of occurrences of which the first are past before the last have taken place; and yet you perceive it as a single movement executed in one present instant. This telescoping of successive events into a single instant philosophers call the "specious present." Doubtless they would assign rather narrow limits to the specious present; but I will willfully make a free use of it, and say that we can extend the specious present as much as we like. In common speech we do so: we speak of the "present hour," the "present year," the "present generation." Perhaps all living creatures have a specious present; but man has this superiority, as Pascal says, that he is aware of himself and the universe, can as it were hold himself at arm's length and with some measure of objectivity watch himself and his fellows functioning in the world during a brief span of allotted years. Of all the creatures, man alone has a specious present that may be deliberately and purposefully enlarged and diversified and enriched.

*b*

No sane contemporary scientist in his investigations of the physical world would disregard nineteenth-century advances in field theory, and no sane historian in his work would rule out of consideration insights achieved in the past century concerning the connection of class conflict with historical occurrences. But this is only to say that all men who are professionally committed to the quest of that elusive entity—the Truth—use all the tracking devices available to them at the time, and in the nature of things cannot use any device before it exists. And of course the adequacy of the historical search at any time is in some degree limited by the adequacy of the tracking devices. In this, too, the historian's situation is no different from that of the scientist. Adequate investigation of optical isomers in organic chemistry, for example, had to wait on the development of the techniques of spectroscopy. If this is what present-mindedness means, then present-mindedness is not just the condition of historical knowledge. For all knowledge at any time is obviously limited by the limits of the means of gaining knowledge at that time; and historians are simply in the same boat as all others whose business it is to know.

Now I do not believe that the proponents of present-mindedness mean anything as bland and innocuous as this. On the contrary I am fairly sure they mean that the historian's boat is different from, and a great deal more leaky than, let us say, the physicist's or the geologist's boat. What then is supposed to be the specific trouble with the historian's boat? The trouble, as the present-minded see it, can be described fairly simply. The present-minded contend that in writing history no historian can free himself of his total experience and that that experience is inextricably involved not only in the limits of knowledge but also in the passions, prejudices, assumptions and prepossessions, in the events, crises and tensions of his own day. Therefore those passions, prejudices, assumptions, prepossessions, events, crises and tensions of the historian's own day inevitably permeate what he writes about the past. This is the crucial allegation of the present-minded, and if it is wholly correct, the issue must be settled in their favor and the history-minded pack up their apodictic and categorical-imperative baggage and depart in silence. Frequently discussions of this crucial issue have got bogged down because the history-minded keep trying to prove that the historian can counteract the influence of his own day, while the present-minded keep saying that this is utterly impossible. And of course on this question the latter are quite right. A historian has no day but his own, so what is he going to counteract it with? He is in the situation of Archimedes who could find no fulcrum for the lever with which to move the Earth. Clearly if the historian is to be history-minded rather than present-minded he must find the means of being so in his own day, not outside it. And thus at last we come up against the crucial question—what is the historian's own day?

## C

The resemblance between elements of **postpositivist psychoanalytic** thought and that of interwar historical **relativists** is striking. **SPENCE'S** turn to a **relativist** orientation in the wake of his disappointed earlier **scientism** parallels **BEARD'S** odyssey. His distinction between the allegedly unmediated access to **reality** of the natural **scientist** and the **analyst** seeing through a glass darkly recapitulates an invalid distinction common to both **BEARD** and **BECKER**. **SCHAFER'S** emphasis on the **pragmatic** purposes for which "everyman" constructs a usable history closely follows **BECKER'S** argument. Comparison between **relativism** in history and in **psychoanalysis** was pursued more systematically by other **psychoanalysts** who reexamined **FREUDIAN EPISTEMOLOGY** and practice. Richard **GEHA**, who went farther than any other **analyst** in adopting a "subjectivist idealist" position, was inclined to invoke **CROCE** and **COLLINGWOOD**. Edwin **WALLACE**, though disavowing a "naïve **Rankean** notion of the facts," took an **objectivist** stance, and generally endorsed the **antirelativist** strictures of **LOVEJOY** and **MANDELBAUM**.

Above all, the resemblance lay in seeking to overthrow the reign of a solely correspondence theory of truth, which had been the bedrock of traditional **epistemology** in both fields, and pressing the claims of its competitors, "**coherence**" and "**pragmatic**" theories. In the traditional view it was assumed that latent **coherence** and **pragmatically** beneficial consequences would naturally follow from an account which "**corresponded**." **SPENCE**, **SCHAFER**, and their allies within **psychoanalysis**, like the historical **relativists**, argued that no possible single account could exactly **correspond**, and that many possible competing accounts could do so "more or less." In their view, while **correspondence** did not necessarily entail **coherence** or utility, **coherence** and utility required a good deal of **correspondence**, since no account not substantially anchored in **reality** could ever, in practice, be judged **coherent**, practical, or in any other sense "true." In their dethroning of **correspondence** as the all-powerful and sufficient criterion of truth, the dissident **analysts** were pursuing the same agenda as their colleagues in other disciplines. Their positive program paralleled that enunciated by **BECKER** in "Everyman His Own Historian." The pasts men created were "as a whole perhaps neither true nor false, but only the most convenient form of error." Each generation "must inevitably play on the dead whatever tricks it finds necessary for its own peace of mind."

of the paragraph. Now in *BIG C* we hear only a quiet fellow whom we saw twice before but did not hear amid the hubbub of the first paragraph. Carl Becker's voice is quiet, but his enunciation is distinct. It is well that it should have been so since it was offering the master account to which historical relativists in America have for more than sixty years rendered due obeisance.

The differences between *BIG C* on the one hand, *small a* and *small b* on the other, are conspicuous. They may, however, obscure a significant similarity among the three. In their divergent ways, all are about how history is and ought to be written. None of them insist that the writing of history should, must, or can be "objective," or "value-free." It is *within* this similarity that the differences of *small a* and *small b* from *BIG C* and their likeness to each other become conspicuous: *small a* makes his points with consummate grace; consciously or not, *small b* seems to be emulating *a*; when finally he gets to his point, *BIG C* with true modesty steps aside and lets *a* make it for him. For, of course, the author of *small a* is that formidable quiet man, Carl Becker, its source is that classic brief for relativism, "Everyman His Own Historian."<sup>7</sup> So, no one will fault the taste of the author of *BIG C* for letting *small a* do his work for him: it is better that way.

<sup>7</sup> Becker, *Everyman*, 240–41.

Meanwhile, one or two readers may have guessed that Professor N. wrote Exhibit C.<sup>8</sup>

But who is the author of *little b*? Surely, he is not a mere clone of the author of *small a*, Carl Becker. Yet between him and Becker there seems to be a sort of genetic affinity. Well, actually, I am the author of *small b*, a page from "The Historian and His Day."<sup>9</sup> In Professor N.'s reading, however, I am a paradigmatic objectivist—vile creature! A perplexity here? Not really. *Small a* and *small b*, Becker and I, are brothers under the thin skin of ideology. We both chose as our vocation the *writing* of history. Having made that choice, it is safer to conjecture that neither of us could ever have written for publication a page like *BIG C*. It is certain we never did.

It does begin to look enough as if my *small b* ways, so manifestly like those of *small a*, Carl Becker, got me a bit closer to him than the *BIG C* ways of Professor N. got him. In the introduction to *That Noble Dream*, Professor N. laments the price he had to pay for "emphasizing breadth of coverage." The price is that "I am unable to offer rounded and nuanced treatments of the thought of the individuals whom I discuss. I have, of course, attempted to avoid misrepresenting their general postures, or overinterpreting their casual remarks or actions, but I am less likely to have succeeded in this attempt than a scholar who has made an intensive study of one or a few individuals. Scholarship, like all of life, is full of trade-offs."<sup>10</sup>

Well, maybe. . . . Still, three years down the road of time, the truism "scholarship is full of trade-offs" may ring a little less true for Professor N. than it did in 1988. When he glances at *BIG C*, Professor N. may wonder whether, in one trade-off he chose, he snookered himself. Anyhow, so much for "Carl Becker, Professor Novick, and Me."

Still, "Cheer Up, Professor N.!" Your last paragraphs dwell ruefully on the uncertainty that must beset any historian as to the worth of his effort. With touching melancholy, you mourn the anarchy in the Israel of professional history. Then you hedge your bet. In 1986 or 1987, chaos reigns at the end of *That Noble Dream*. But, you wondered, would it last forever? It did not; your prudence has paid off handsomely.

Three or four years later; Exhibit *D* (1990); it is a digest of a recent manifesto on the right way to do history in primary and secondary schools. The issue is a proposed history curriculum. Does *D* address the "objectivity problem"? That is hard to say; the term is not present. It does address itself to "standards of accuracy and integrity, the commonly accepted standards of evidence, honest and conscientious scholarship" in historical inquiry, the kind of scholarship of which *That Noble Dream* is an admirable exemplar. And it also expresses a certain naïve enthusiasm for "the Western tradition" of "individual freedom and political democracy." Unwisely perhaps, but with no hedging, and eyes at the time not fixed on the intellectual-chic gauge, I spent the years 1984–1989 as director of a Center for the History of Freedom, and as editor of the first volume of a series,

<sup>8</sup> Novick, *That Noble Dream*, 562–63.

<sup>9</sup> J. H. Hexter, "The Historian and His Day," in *Reappraisals in History* (London, 1961), 4–5.

<sup>10</sup> Novick, *That Noble Dream*, 8–9.

## D

The history taught to . . . children must meet the highest standards of accuracy and integrity. We steadfastly oppose the politicization of history, no matter how worthy the motive . . .

In July, 1989, a task force on minorities, appointed by the New York commissioner of education, submitted a report . . . calling for revision of the history curriculum . . .

The report, a polemical document, viewed division into racial groups as the basic analytical framework for an understanding of American history. It showed no understanding of the integrity of history as an intellectual discipline based on commonly accepted standards of evidence. It saw history rather as a form of social and psychological therapy whose function is to raise the self-esteem of children from minority groups.

The Regents endorsed the report and authorized the revision of the history curriculum by a panel of 21 persons. Of this group six to eight are to be scholars distributed among seven fields; the panel might well end up with only one historian. "Care will be taken," the Regents add, "to ensure that among the active participants will be scholars and teachers who represent the ethnic and cultural groups under consideration"—which sounds like an invitation to each group to write, or veto, its own history.

[We] are, we believe, well known for [our] commitment to equal rights and . . . rejection of any form of racism in the schools and in society. We are also united in our belief in a pluralistic interpretation of American history and our support for such shamefully neglected fields as the history of women, of immigration and of minorities.

We have an equal commitment to standards of historical scholarship. We condemn the reduction of history to ethnic cheerleading on the demand of pressure groups. And we reject as unfair and insulting the implicit assumption in the . . . report that minorities are incapable of absorbing a first-class education.

We have a further concern: The commissioner of education's task force contemptuously dismisses the Western tradition. Recognition of its influence on American culture, the task force declares, has a "terribly damaging effect on the psyche" of children from non-European cultures. No evidence is adduced to support this proposition and much evidence argues against it.

The Western tradition is the source of ideas of individual freedom and political democracy to which most of the world now aspires. The West has committed its share of crimes against humanity, but the Western democratic philosophy also contains in its essence the means of exposing crimes and producing reforms. Little can be more damaging to the psyches of young Blacks, Hispanics, Asians and Indians than for the State of New York to tell them that the Western democratic tradition is not for them.

And little can have more damaging effect on the republic than the use of the school system to promote the division of our people into antagonistic racial groups. We are after all a nation—as Walt Whitman said, "a teeming Nation of nations"—and history enables us to understand the bonds of cohesion that make for nationhood and a sense of the common good: *unum e pluribus* . . .

We will insist that the . . . history curriculum reflect honest and conscientious scholarship and accurately portray the forging of this nation from the experiences of many different groups and peoples.

[C]hildren . . . deserve no less than the best.

The Making of Modern Freedom. Freedom is a phenomenon as Occidental in origin as Confucianism is Far Eastern. I expect that volume to attend the AHA meeting next year even if I do not make it.

During the decades of your scholarly labors (1967–1987?), the West and its freedom and democracy were not intellectually chic. Nor was the view that historians were bound to keep their eyes wide open for bias, carelessness, and outright cheating in all the sources they used. That is perhaps the view of those

naïve, deluded fogies from thirty-six to eighty-eight years of age who signed off on Exhibit D.<sup>11</sup> It is certainly mine. And, in practice, it is palpably yours.

Taken all in all, a question arises about the historiographic turbulence that so impressed you, Professor N., in about 1987. Is it more than a generational propensity to world-historical *mal de mer*? Perhaps the fogies committed to Exhibit D might not find your argument here and now entirely compelling. Nor indeed do I.

Your scriptural tag, "Every man did that which was right in his own eyes" (Judges 21:25), is apt—relatively, that is. It neatly applies to the Israelite tribes in the Middle East bashing the Benjamites in the twelfth century B.C. Does it fit professional historians in America in 1990? You tell us all these historians have long been stumbling in the dark. Really? Take courage, my colleagues. "Westward, look, the land is bright." And, if you can, cheer up Professor N.!

<sup>11</sup> Diane Ravitch and Arthur Schlesinger, "Remaking New York's History Curriculum," *New York Times*, August 12, 1990.



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*AHR Forum*  
Comments on *That Noble Dream*

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LINDA GORDON

WE ARE ALL IN PETER NOVICK'S DEBT for this book. It is a gift to historians and other scholars, present and future, and the labor and intelligence crystallized in it could not easily be surpassed. Oddly, it is an achievement that we can all take pride in, wherever we stand in relation to Novick's interpretations, for it represents so well the richness of our discipline's contributions. Given what a vast compilation he has created, and what a good storyteller he is, it is all the more astonishing that the book is so thoughtful and insightful. If the book makes many of us begin to dream of other books that should be written—that we might want to write—that is further testimony to its value.

With a view to stimulating substantive discussion, I want to limit my praise for this work—which could be extensive and time-consuming—in order to concentrate on two differences I have with Novick. First, it seems to me that the fundamental question and organization of Novick's approach tend to reify a dichotomy—objectivity/relativism—which only partly characterizes the approaches of historical scholarship over the past century. Second, I believe that his approach privileges an epistemological issue, when his own evidence suggests that the present-day political agendas of historians were often the more fundamental determinants of their apparently theoretical choices. The evidence suggests that historians got excited about the objectivity question when it was mobilized in the service of political conflict.

In general, the method of the book is to look for the abstract theoretical statements of historians (and other scholars who influenced historians) regarding objectivity. Novick's implicit definition of what counts as theory, privileging metahistorical rather than historical statements, has been challenged lately, particularly by scholars from groups traditionally excluded from high learning who may choose different forms of discourse. Novick's book may provide yet another example of the distortions produced by such a definition. It seems likely that abstract theoretical statements tend toward more simplified, pure expressions of positions, and indeed Novick may have sought out the more extreme positions. Many historians—and this includes those whose theoretical views are studied in the book—use more complex, nuanced, even ambivalent and contradictory, assumptions regarding objectivity in their actual historical writing and teaching.

There is room for ambivalence about the very question of whether historians *should* articulate their epistemological assumptions abstractly. I have regretted the

silence of many historians regarding their claims to truth. At the same time, there are values in historians' lack of self-consciousness about their approaches, because their "naïve" narrative, descriptive, or expository writing more often retains a readability that has been generally lost among other disciplines. In one of the few issues on which I agree with my colleague Ted Hamerow, I lament the increasing specialization and sub-jargons within history writing and cherish the work of those historians who remain dedicated to communicating with a general educated public. Our abilities to tell a good story, to bury interpretation within narrative and other discourse, draw nonspecialists to our work. (Those who are convinced that this public, civic, style has been entirely lost are mistaken because they are uninterested in the areas of history that continue that tradition—labor history, women's history, minority history, yes, even "old-fashioned" political history.) At the same time, we must recognize that this silence about theoretical assumptions makes critique more difficult and nourishes erroneous assumptions about where bias and objectivity are located.

I would argue that the more interesting debates among historians have been between more moderate views regarding objectivity or relativism. Few historians believe that they play no interpretive role; few believe that any interpretation is as good as any other. For example, the prevailing "standpoint" theory is not at all relativist. It argues that individuals are constrained in their insights by their social positions; and that, other things being equal, some social positions produce better views of certain topics than others—for instance, that women, as the subordinated sex, are better positioned to examine gender relations. Many proponents of such a theory would accept that women scholars could be wrong; that male scholars could correct women's mistakes about women's history. Similarly, the notion of "situated knowledge" as it has been developed does not imply that there is an infinity of situations and different interpretations, because it refers not to individual but to large-scale social situations, which can be fairly precisely described and categorized. And most historians would agree—in their actual historical writing if not in their theoretical musing—that, while it may be difficult to label a particular rendition definitive, it may be easy to label various renditions wrong.

In fact, attention to the experience of the craft of historical scholarship itself might suggest why few historians cling to purely objectivist or relativist positions. The work historians must do in order to find texts, which they make into evidence, inclines some to a sense that the sources speak to us. I have experienced research as requiring me to be very quiet when reading documents so that I can "hear" them speak. Thus I interpret the discomfort of many historians with theorizing about their role as having to do with this discipline, so valuable, of silence and of aloneness with pieces of paper (perhaps also pottery shards, etc.). Certainly, this intimacy with sources makes us feel at times like mediums, merely transmitting the past by making ourselves empty. The research experience is also often marked, however, by the fragmentary nature of sources. I assume that many historians, like myself, actually *like* this very incompleteness, not only because of the excitement of discovery but also because it licenses interpretation.

Novick's focus on philosophizing about history distances us from this sense of the process of history writing and the epistemological sensibility it helps create.

Indeed, while objectivity is certainly at issue, I am not at all clear that relativism is the correct formulation of the critique of objectivity. I see rather a continuum between objectivity and *interpretation*, with many energetic disputes about the *nature* of interpretation. Poststructuralist thought enables us to see one such dispute: between deconstructionism and what one might call social/historical constructionism. Both deny that meanings are immanent in sources; both direct important attention to changing and contextual meanings of language. Radical deconstructionists find the creation of meaning in the act of reading and theoretically allow an unlimited range of readings/meanings, because of their skepticism about unified subjects, let alone unified social subjects. A more traditional social/historical theory of interpretation sees meanings as flowing from historical cultures surrounding both sources and their interpreters: an interpretation would be wrong if it projected anachronistic interpretations onto historical artifacts/texts; yet we can never safely assume that present-day cultures can be filtered out of our readings of the past. The tension between these two opposite pitfalls defines wonderful intellectual tasks.

Moreover, Novick's choice to use objectivity/relativism as an overarching organization subordinates and thus somewhat oversimplifies the meanings of the tension between structuralist determinism and emphasis on agency (of all kinds—bottom-up resistance, the influence of great individuals, for two examples). While there have been intense criticisms and counter-criticisms among historians regarding their emphases on structure or agency, hegemony or dissidence, social or individual influences, most positions have not been at the extremes.

Deconstructionism may have had a greater impact in my field—women's history—through the hegemony of that method in literature and feminist criticism. It has had a revivifying effect on self-consciousness about our assumptions (although it is surely vulnerable to lack of self-consciousness about its own assumptions). When used well, it tends to raise questions about how questions are posed, one of the most important and difficult techniques of critical thought. It has led to wonderfully subversive critiques of just such binary choices as objectivity/relativism. Yet it has also been used to reify such dichotomies as male/female or black/white. My quarrel with deconstructionist advocates is about their claims for the novelty of what they are doing, not about the value of questioning our access to "reality."

Dichotomizing objectivity and relativism thus minimizes other important tensions, some of which are not binary at all: about relations between social being and scholarly consciousness; between structure and agency; among varieties of history; between finding and constructing meaning, for example.

ONE OF THE SUBORDINATED TENSIONS in Novick's account is between "Left" and "Right." To be sure, one of the book's contributions is in compiling evidence of how heavily such political orientations have weighed in the profession at times,

and Novick points out that “schools of historical interpretation are never politically neutral.”<sup>1</sup> But the evidence is scattered and not schematized. Novick shows that objectivity and relativism never correlated consistently with these political divisions but have cut across them. Still, I think we must question which division has been the more independent variable. It seems to me that, on the whole, the strongest proclamations of objectivity have been made by center, dominant groups and criticized by marginal groups; that Novick’s evidence shows this; and that he nevertheless draws back from the implications of this finding.

This is particularly vividly exemplified by contemporary academic politics. As conservative academics organize against affirmative action and for their canon of what is central to history teaching and writing, they claim objectivity. They defend that claim by a *reductio ad absurdum* of a relativist position and accuse their opponents of fashionableness, thus avoiding substantive discussion of what is important and should be taught in history. In fact, supporters of affirmative action and critics of the “western” civ. canon are not usually relativists but critics of the conservatives’ claimed objectivity. The contending epistemological position is more complex and nuanced than relativism, but Novick does not examine it. Novick’s approach more obscures than illuminates the actual theoretical positions of those who, for example, think that African-American history is part of U.S. history, that conditions in the “Third World” are part of the history of Europe, that the construction of gender and family are fundamental to modern politics and economies. These may actually be objectivist, not relativist views; by using “relativist,” the pejorative term of the objectivists, to describe their opponents, Novick avoids discussing what the nonobjectivists do think.

The influence of politics is obscured by Novick’s objectivity/relativism formulation, his concentration on positions at the extremes, and his focus on abstract rather than applied theories. By their nature, most critiques of previous historical scholarship are at least somewhat objectivist in labeling earlier versions wrong, not just different. And most critiques have attempted to explain the error of earlier work in terms of the social sources of that earlier interpretation, making the critiques also somewhat relativist. Furthermore, most critiques are influenced not only by societal politics but by intra-professional power struggles as well.

The influence of politics may also be obscured by Novick’s definition of what is “Left” and what is “Right.” In general, we might identify two types of definitions of the Left: a more traditional one, on which Novick usually relies, referring to Marxist or other anticapitalist views; and a newer, broader one, referring to all openings toward greater democracy and thus toward the inclusion and representation of marginalized social groups and toward the “new social movements.” But even when there is no clear Left/Right definition, one must still see “politics,” by which I mean the organized struggle of different social groups for power, including the struggle on the part of those who already have power to keep it.

Novick’s definition becomes particularly unsatisfactory when dealing with the last few decades. By considering women’s and minority history under the category “particularist” consciousness—which is not the same as relativism—he avoids the

<sup>1</sup> Peter Novick, *That Noble Dream: The “Objectivity Question” and the American Historical Profession* (New York, 1988), 458.

challenge of this work. In fact, critiques and defenses of particularism go on within these fields. And precisely the point of Nell Painter's "Who Decides What Is History?" was to change the definition of the whole field. Women's and minority historians have been concerned with the fundamental definitions of what counts *as* history, not only *in* history. Their critiques not only consider the exclusions of women and minorities but ask for reconsideration of how the most important themes of U.S. history are conceptualized, such as religion, republicanism, citizenship, political party construction, the frontier, for example.

Indeed, I cannot avoid noting that Novick's treatment of women's history is so scandalously misinformed that it becomes disrespectful. Feminist scholars are treated quite differently from scholars of any other political persuasion: not primarily as scholars or intellectuals at all but as political activists. Of the nineteen pages devoted to women's history, no more than three discuss the historical or theoretical work of women's historians. The rest is about a much-publicized political conflict—the *Sears* case—and the politics of women's studies programs. (Perhaps I should point out that almost all women's historians work in history departments, not in women's studies programs.) Novick misses not only the rich theoretical debates within women's history—about structure and agency, about the possibility of postulating any cross-class and cross-race shared interests among women, about the meaning of public and private, for example—but he also misses the academic politics.

Indeed, from feminist theory has come a critique of claims of objectivity of a specifically political nature: viewing objectivity as a claim created by a male stream of thought. This has been argued in various theoretical frameworks (object relational, Lacanian, deconstructionist). I am skeptical of this phenomenology, because I am skeptical about all claims of consistent and thoroughgoing sex difference. Nevertheless, the gender system that creates in women greater sensitivities to relationship may be expected to have some influence on scholarly relations to historical subjects.

Ultimately, I would suggest that the crucial question for historians is how the debate over objectivity affected actual history writing. I am suggesting, of course, another book—I admit it—and would hardly expect Novick to take on another such massive task. But I would point out that this critical question cannot be answered without looking at the possibility that these metahistorical theoretical claims were wielded primarily as weapons in the politics that history writing usually contains.

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*AHR Forum*  
Postmodernist Theory and *Wissenschaftliche* Practice

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DAVID A. HOLLINGER

ONE OF THE BEST PASSAGES in Peter Novick's marvelously written book expresses Novick's understanding of his subject. "I spend a good deal of time talking about what historians do worst, or at least badly: reflecting on epistemology . . . [It is] something like a sportswriter reporting on [historians'] performances in the annual history department softball game."<sup>1</sup> As concerns the intellectual history of the "objectivity question," this is exactly right. Novick has written a history of the theoretical discourse of a community of non-theorists. A study of donut holes? No; far from it.

The theoretical discourse of our community of non-theorists is eminently worth studying. From it, we learn a great deal concerning the professional culture of academic historians in the United States and about the terms on which historians have participated in, and/or resisted, the successive enthusiasms and preoccupations of American academic intellectuals generally. Hence *That Noble Dream* is a strikingly successful contribution to the field of American academic history. Novick tells us more about the philosophical presuppositions, ideological predictions, social biases, and professional norms of the discipline he studies than does any work known to me addressed to any one of the major social scientific and humanistic disciplines practiced in the United States during the same time frame.

Although Novick is sometimes criticized for not defining the "objectivity question" more sharply, and for not organizing his book more rigorously around debates concerning this question, I believe the book is all the more valuable because Novick has construed this question so broadly as to enable him to follow his own instinct for what is interesting about the development of the profession. *That Noble Dream* is even more useful considered as a social and political history of the profession than it is considered as an intellectual history of the profession's discourse about objectivity.

Novick has given us a much-needed critical introduction not to the historian's craft, not to the philosophy of history, but to the standing lore of the profession. It is ideal reading for new recruits as they enter graduate school. I have assigned it in three different seminars of beginning graduate students and find that the book generates an appropriate mixture of gasps of horror and of self-recognition.

<sup>1</sup> Peter Novick, *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (New York, 1988), 15.



It also generates some of the most probing and animated seminar discussions it has been my privilege to lead in twenty years of graduate teaching.

Some of what Novick says about the development of our professional culture invites challenge, but for the most part his account is convincingly documented, in an old-fashioned, *wissenschaftliche* way. Stories that used to be handed down from generation to generation in the faculty lounge and at semester-end parties for graduate students are now the stuff of footnotes based on multi-archival research: Novick has gone to the archives to find out if John D. Hicks really did express alarm at Armin Rappaport's possible appointment at Berkeley on account of Rappaport's being Jewish and from New York, and hence a left-winger. So it is with hundreds of items of what used to be gossip, as well as with many hundreds more that were never even matters of gossip but have been discovered afresh by Novick in the course of his research. *That Noble Dream* is a suitable primer for recruits to the profession not only on account of its subject matter but also on account of its indefatigable devotion to some of our standard professional ideals, including empirical verification and a generous measure of critical detachment. Novick's achievement, then, is of a fairly traditional sort and was suitably recognized by our Association a year ago through the awarding to Novick of the Beveridge Prize.

The limits of Novick's achievement come into view, however, when we attempt to use his book to help us figure out just where the "objectivity question" is today, or should be. It is hard for me to take seriously Novick's claim that the "discipline of history" had "ceased to exist" by the 1980s and that "convergence on anything, let alone a subject as highly charged as 'the objectivity question,' was out of the question."<sup>2</sup> I do not think the profession finds "the objectivity question" so highly charged. Given a little provocation, we seem ready to drop whatever else we may be doing to run to New York to argue about objectivity. Indeed, the publication of Novick's own book seems to have created within the historical profession a discourse about "objectivity" of just the sort that Novick says had become impossible. Witness the probing and thoughtful essay-reviews by James Kloppenberg in the *AHR*, by Thomas Haskell in *History & Theory*, and by Kenneth Cmiel in *American Literary History*.<sup>3</sup>

If, during the decade prior to the provocation of Novick's book, historians had not been saying much about the objectivity question, perhaps this relative silence derives not from this question's being too hot to handle, as Novick suggests, but from the fact that this question has left a lot of folks rather cold. So many other issues within the research programs of our own specialized fields of historical scholarship have seemed a lot hotter, and more engaging. I am intrigued by Alan Brinkley's observation that objectivity "seems to be one of the few things about which something like a consensus appears to have emerged within the historical

<sup>2</sup> Novick, *That Noble Dream*, 628.

<sup>3</sup> James T. Kloppenberg, "Objectivity and Historicism: A Century of American Historical Writing," *AHR*, 94 (October 1989): 1011–30; Thomas L. Haskell, "Objectivity Is Not Neutrality: Rhetoric vs. Practice in Peter Novick's *That Noble Dream*," *History & Theory*, 29 (1990): 129–57; Kenneth Cmiel, "After Objectivity: What Comes Next in History?" *American Literary History*, 2 (1990): 170–81.

profession.”<sup>4</sup> I suspect Brinkley exaggerates the extent of agreement, but Brinkley is surely right to insist that historians have found other questions more engaging.

Among the historians who have found other questions more engaging is Peter Novick himself. Novick makes clear in his early pages that his book makes no argument on the “objectivity question” as such, offers no answer to it: “this isn’t that sort of book,” Novick says candidly and honestly.<sup>5</sup> Yet, toward the end of the book, Novick acts as though he had made such an argument, and triumphantly. The voice he adopts in the final chapter, “There Was No King in Israel,” is a more sweeping, less patient voice than he has used in the earlier chapters. He leaves the impression that the ideal of objectivity, even in its most hermeneutically self-conscious reformulations, has been decisively refuted and what is needed is simply the Nietzschean courage to face a relativist abyss. Thomas Haskell and I get depicted in the last few pages as well-meaning liberal saps, nice fellows who just happen to be utterly oblivious to how anachronistic our ideas have become. It almost seems that Haskell and I are to the myth of objectivity what Haldeman and Ehrlichman were to the Nixon White House, two guys haplessly trying to “save the plan” while our world crumbles.<sup>6</sup>

Although I am tempted to use this occasion as an opportunity to explain why I believe this shoe does not fit, and why the attendant vision of our current historiographical situation is misleading, I am resisting this temptation for two reasons. First, many of the points I would make have already been made in the reviews published by Kloppenberg, Haskell, and Cmiel. Second, I am eager to shift the discussion of *That Noble Dream* away from the contemporary theoretical issues about which Novick has little to offer us and toward the historical issues about which Novick has so very much to offer. If we want help in formulating and addressing theoretical issues, there are plenty of professional colleagues who can help us more than Novick can. We can look, for example, to Frank Ankersmit, to Robert F. Berkhofer, Jr., to Leon J. Goldstein, to Martin Jay, to Dominick LaCapra, to Allan Megill, to John Toews, and to a number of others who write about these issues in *History & Theory* and elsewhere.<sup>7</sup>

By way of trying to advance this shift, I want to present two lists of representative questions, one consisting of questions concerning which I believe Novick helps us little even though his book is sometimes taken to include answers to these questions, and a second consisting of questions concerning which Novick has a lot to say deserving of more critical scrutiny than we have been giving it.

<sup>4</sup> Alan Brinkley, review of Novick, *That Noble Dream*, in *Times Literary Supplement* (November 10–16, 1989): 1246.

<sup>5</sup> Novick, *That Noble Dream*, 6.

<sup>6</sup> Novick, *That Noble Dream*, 625–28.

<sup>7</sup> See, as examples of the relevant recent literature, the following: Frank Ankersmit, “Historiography and Postmodernism,” *History & Theory*, 28 (1989): 137–53; Robert F. Berkhofer, Jr., “The Challenge of Poetics to (Normal) Historical Practice,” *Poetics Today*, 9 (1988): 435–52; Leon J. Goldstein, “Impediments to Epistemology in the Philosophy of History,” *History & Theory*, Beiheft 25 (1986): 82–100; Martin Jay, *Fin-de-Siècle Socialism and Other Essays* (New York, 1988), esp. 17–63; Dominick LaCapra, *Rethinking Intellectual History: Reappraisals and New Perspectives* (Ithaca, N.Y., 1983); Allan Megill, “Recounting the Past: ‘Description,’ Explanation, and Narrative in Historiography,” *AHR*, 94 (June 1989): 627–53; John Toews, “Intellectual History after the Linguistic Turn: The Autonomy of Meaning and the Irreducibility of Experience,” *AHR*, 92 (October 1987): 879–907.

So, first, a representative list of questions concerning which we should look elsewhere, beyond Novick: Did Carl Becker really say it all? In just what sense of the word, if any, is “objectivity” still a valid ideal for historians? Is history a type of fiction? How do the books and articles produced by historians compare in epistemological status to the artifacts in which novelists, literary critics, sociologists, geologists, and physicists present the results of their work?

Turning to my other list, a list representing questions concerning which Novick does have important things to say, deserving of our appreciation and our critical scrutiny: When the ideal of “objectivity” has been invoked by certain historians at certain times and places, what did they mean by it? How can we explain the ups and downs of certain components of the “myth of objectivity” in terms of the political, ethno-demographic, and economic conditions that have impinged on the historical profession of the United States? How have these same political, ethno-demographic, and economic conditions influenced the behavior of historians toward one another, irrespective of the “objectivity question”? How has the vocation of history—and the profession organized to promote and protect that vocation—been understood by those historians with responsibility for running the major institutions of the profession, i.e., the associations, the journals, and the academic departments? What happened to the historical profession in the 1960s? How did Richard Hofstadter handle Mark Naison’s Ph.D. oral? Do historians who are Jewish have a more sympathetic “take” on African-American history than do historians who are Anglo-Saxon Protestants? Why is Wallace Notestein not acknowledged in the preface of J. H. Hexter’s *Reign of King Pym*?

It would be easy to extend this second list, which I take to illustrate not so much issues about which Novick is obviously correct in every detail, but issues, small and large, about which Novick has something serious and interesting to say.

I would, however, be happy to hear Novick address more directly than he has so far just what he takes to be the relationship between these two kinds of questions. He tells us that he has been more persuaded by the critics of the objectivity myth than by its defenders,<sup>8</sup> yet he has written a book ideally suited to please the latter. This is a very traditional monograph attentive in the extreme to standard professional norms. There may not be a paradox here, but if there is not, it would be good to have explained more fully why there is not. If an “argument” can possess “relative autonomy . . . from details of the evidence” as Novick tells us was true in the case of David Abraham’s much-debated study of Weimar industrialists and Nazism,<sup>9</sup> why does Novick work so hard to document his own claims, rendering them less vulnerable to attack than were the claims of David Abraham? Does Novick’s *wissenschaftliche* practice imply that doubts about the objectivity myth need produce no changes whatsoever in the way we do history? If the concept of objectivity is, as Novick tells us, “essentially confused,”<sup>10</sup> so what?

The closest thing to a response to this question I can find in *That Noble Dream* is Novick’s characterization of the opening lines of the Declaration of Indepen-

<sup>8</sup> Novick, *That Noble Dream*, 6.

<sup>9</sup> Novick, *That Noble Dream*, 617.

<sup>10</sup> Novick, *That Noble Dream*, 6.

dence as “salutary nonsense.” The ideals set forth in the Declaration of Independence are as confused and incoherent, it seems, as are the traditional ideals of historians, but Novick professes to appreciate the role of natural rights concepts “as bulwarks of liberty and equality.” Yet in the next paragraph Novick quotes Isaiah Berlin’s formulation of the Hegelian insight that even “great liberating ideas” turn “inevitably” into “suffocating straightjackets.”<sup>11</sup> Who is in the strait jacket? Those Americans who still believe in human rights? Those historians who still try to persuade their colleagues on the basis of empirical evidence and logical reasoning? Novick himself?

Although I will be glad if Novick can tell us more about what he takes to be the relation between his practice and his theory, I want to underscore my earlier emphasis on the success of that practice. In concluding with this appreciation for Novick’s achievement, I suppose I am confirming one facet of Novick’s characterization of me in the final pages of *That Noble Dream*: there, he describes me as someone given to “accentuating the positive.”<sup>12</sup> When it comes to talking about *That Noble Dream*, I propose to continue doing just that.

<sup>11</sup> Novick, *That Noble Dream*, 7.

<sup>12</sup> Novick, *That Noble Dream*, 625.

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*AHR Forum*  
Fragmentation and the Future of Historiography

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ALLAN MEGILL

MY AIM IS TO POINT OUT some broad, and I think little-pondered, implications of the account of the American historical profession that Peter Novick offers in *That Noble Dream*. In particular, I propose to focus on the fourth and final part of the book, entitled "Objectivity in Crisis." There are four chapters in Part IV, bearing the following descriptive titles: "The Collapse of Comity"; "Every Group Its Own Historian"; "The Center Does Not Hold"; and "There Was No King in Israel."

Most professional historians, in reading these headings and the accounts to which they apply, will be inclined to see Novick as portraying a situation that is primarily negative in its implications. For example, in a long review of *That Noble Dream*, James Kloppenberg asserted that "[i]n his conclusion, Novick laments that by the 1980s, 'there was no king in Israel,' and, as a result, 'every man did that which was right in his own eyes.'"<sup>1</sup> In a draft response to Kloppenberg that Novick sent to me in February 1990, after I wrote to him asking for a list of the most recent reviews of his book, Novick conceded that in quoting Judges 21:25 he had committed a "serious rhetorical gaffe," for his actual assessment of the situation in professional historiography was not "apocalyptic," nor did he mean to suggest that contemporary professional historiography was in a state of "individualistic anarchy."<sup>2</sup> In fact, while "there was no king in Israel" characterizes quite well the state of our discipline, "every man did that which was right in his own eyes" does not. A careful reading of Novick's book does not suggest that he has an apocalyptic view of the current fragmented state of historiography, nor does it suggest that he "laments" that state. If anything, the lament is Kloppenberg's; it is not Novick's.

I find that I am profoundly suspicious of attempts to overcome the fragmentation, of attempts to restore (at some higher or more sophisticated level) the synthesis. Belief in the virtue of synthesis and in the badness of fragmentation seems deeply ingrained within our academic culture and within the culture of professional historians in particular. Every few years, we find proposals being advanced for some new synthesis or other.<sup>3</sup> Let us be warned, however: all calls

<sup>1</sup> James T. Kloppenberg, "Objectivity and Historicism: A Century of American Historical Writing," *AHR*, 94 (October 1989): 1011–30; quote at 1029, citing Peter Novick, *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (New York, 1988), 628.

<sup>2</sup> Novick returns to the point in "My Correct Views on Everything," *AHR*, 96 (June 1991): 702.

<sup>3</sup> The most recent proposal to be widely noticed in the profession is offered by Thomas Bender, "Wholes and Parts: The Need for Synthesis in American History," *Journal of American History*, 73

for synthesis are attempts to impose an interpretation. It is fair enough to argue for a particular interpretation *as* an interpretation. But it will not do to present a particular interpretation as the synthesizing magic thread. I find no justification—certainly no articulated justification—for taking “fragmentation” as a dispraising term and “synthesis” as a praising one. We can hope to attain clarity of mind on such issues only if we take these terms as neutral.

Such is the power of academic professionalism that even in those fields that have contributed most to the fragmentation, lip service continues to be paid to the ideal of unity. For example, in a review of five recent articles in women’s history, part of a very interesting *AHR* forum, Kathryn Kish Sklar points to the “notable assets” that have come with the growth of women’s history and then observes that “we must nevertheless recognize that our current situation contains all the liabilities associated with rapid growth, especially inadequate integration.” But is “inadequate integration” a liability, as long as insight is advanced in other ways? It is not surprising to find that Sklar follows this observation with a plea for her own “paradigm” for understanding women’s movements in different countries.<sup>4</sup> Sklar’s way of explaining the emergence and development of women’s movements may have many virtues; it may indeed “integrate” our understanding of those movements. But it would be a mistake to take the possible future success of Sklar’s integration as reflecting negatively on those accounts of women’s movements that the integration fails to accommodate. Other historians may be preoccupied by other issues. Accordingly, the questions to ask of their work are: How interesting are these issues? How well are they addressed? Judgment of quality would come from the answers to these critical questions, not from whether the works conform to Sklar’s or anyone else’s “paradigm.”<sup>5</sup>

In the same symposium, Daniel Walkowitz suggests in an article on social workers in the 1920s that “[t]o tell the full story of twentieth-century social workers’ search for identity, historians need to draw on the literatures of consumption, work, and professionalization, for the development of social work

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(June 1986): 120–36, with responses by Nell Irvin Painter, Richard Wightman Fox, and Roy Rosenzweig and a response to the responses by Bender, published as “A Round Table: Synthesis in American History,” *Journal of American History*, 74 (June 1987): 107–30. On the deep roots of the longing for synthesis, see the discussion of the “bias for universality” in Allan Megill, “Recounting the Past: ‘Description,’ Explanation, and Narrative in Historiography,” *AHR*, 94 (June 1989): 627–53, at 632–36, 650–51, and *passim*. Compare Ludwig Wittgenstein on “our craving for generality,” in Ludwig Wittgenstein, *The Blue and Brown Books* (Oxford, 1958), 17–19. Of course, I do not deny the role that theoretical unification plays in explanation (see the classic discussion by Carl G. Hempel, “The Function of General Laws in History” [1942], in Patrick Gardiner, ed., *Theories of History* [New York, 1959], 344–56). I diverge, however, from the strict logical empiricist view that Hempel offers, since it does not adequately take account of the practices of history writing.

<sup>4</sup> Kathryn Kish Sklar, “A Call for Comparisons,” *AHR*, 95 (October 1990): 1109–14, quote at 1111.

<sup>5</sup> The term “paradigm” needs, in any case, to be viewed with suspicion. All too often, it is defined in a loose way that invites a usually unnoticed slippage from the notion of an “explanatory theory” to the much broader notion of an “interpretive perspective.” The term goes back to Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago, 1962). Margaret Masterman, “The Nature of a Paradigm,” in Imre Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge* (Cambridge, 1970), 59–89, pointed out the multiple meanings of the term in Kuhn’s study. Not surprisingly, in returning to these matters in the second edition, Kuhn substituted for “paradigm” the more variegated notion of a “disciplinary matrix”; see Thomas S. Kuhn, “Postface—1969,” in Kuhn, *The Structure of Scientific Revolutions*, 2d edn., enlarged (Chicago, 1970), 174–210, esp. 183–87.



as a profession was shaped by cultural conventions and limited by the material realities of the home and the workplace.”<sup>6</sup> Walkowitz identifies elements that historians would certainly want to find in an account of “social workers’ search for identity.” But what justifies the claim that these elements would lead us to “the full story” of that search? The justification, I suggest, lies deeply embedded in the professional identity of historians. Walkowitz’s use of the phrase “the full story” is almost certainly offhanded, but the offhandedness makes his use of it all the more significant, all the more a marker of a widely shared historians’ bias.

The bias, it seems to me, is currently under challenge. Novick’s remarkable book is one of the elements in that challenge, for in his wide-ranging, ironic, dispassionate—indeed, in several senses of the term, *objective*—account of the American historical profession, he calls into question precisely those notions of objectivity that lie hidden in the idea that there is a “full story.”

The most sophisticated observers of the historiographical scene understand well enough the contingency of those faiths that hold professional historiography together. With contingency comes the alleged threat of fragmentation. The sophisticated response to the alleged threat is the pragmatic, Peircean appeal to “communities of the competent.”<sup>7</sup> But this will not do. There is a disciplinary blindness that prevails within the modernist academy, and not only among historians. This is the blindness of historians who argue only with other historians, philosophers only with other philosophers, economists only with other economists, and so on. When one’s universe of argument is restricted in this way—and the disciplinary structure of the modernist university certainly encourages such restriction—it is easy to imagine that one knows what competence is.

But there is no single competence. An argument deemed acceptable by the consensus of competent historians may well be deemed unacceptable by the consensus of competent philosophers or economists—and vice versa. Many historians have in fact never entered into serious argumentative relation with economists or philosophers or literary theorists or rhetoricians, for such relations are not in general given professional reward. Hence they do not know how multifarious is competence. That the “community of the competent” argument has been taken seriously is one marker of the firmness of disciplinary divisions within our institutions of higher learning.<sup>8</sup> Novick’s account of raging controver-

<sup>6</sup> Daniel J. Walkowitz, “The Making of a Feminine Professional Identity: Social Workers in the 1920s,” *AHR*, 95 (October 1990): 1051–75, at 1074.

<sup>7</sup> The response is most closely associated with Thomas Haskell. See especially Thomas Haskell, “Professionalism *versus* Capitalism: R. H. Tawney, Emile Durkheim, and C. S. Peirce on the Disinterestedness of Professional Communities,” in Thomas Haskell, ed., *The Authority of Experts* (Bloomington, Ind., 1984), 180–225, esp. 207. Essentially the same position is to be found in the writings of a number of other historians who likewise appeal to a disciplinary consensus that would overcome conflicting positions. See, for example, David Hollinger, “T. S. Kuhn’s Theory of Science and Its Implications for History,” in David Hollinger, *In the American Province: Studies in the History and Historiography of Science* (Bloomington, Ind., 1985), 105–29, at 124, 128–29; and Kloppenberg, “Objectivity and Historicism,” 1029. For Novick’s runthrough of these issues, see *That Noble Dream*, 570–72 and 625–28.

<sup>8</sup> Lest I be misunderstood, I am not arguing for interdisciplinary unification. The more that one knows, through the experience of having argued with them, about how the practitioners of other disciplines argue, the less likely one is to think that the different modes of argument are compatible enough for any one person to practice them at the same time. Thus I am deeply suspicious of notions of convergence between different disciplines.

sies among historians, and of the now-discarded assumptions of earlier generations of historians, needs to be set within this wider socio-intellectual context.

And yet, professional identity has been important for the development of historical knowledge. As one part of his very large story, Novick shows that the repudiation in the post-World War II period of the relativist critique of objectivity offered by Charles Beard and Carl Becker in the 1930s was closely connected with the conception of history as "an autonomous profession."<sup>9</sup> "Autonomy," like "synthesis," is another of those words to which most professional historians, without articulated justification, attribute positive value. Thus when Novick observes that for most women's historians, "the feminist community was at least as salient a reference group as was the profession,"<sup>10</sup> he is likely to be read as saying something bad about women's history. But such a reading of Novick seems to me to be quite wrong. Novick does not approve of "autonomy," nor does he disapprove of it. On the contrary, here and elsewhere he seems to me to be determinedly neutral on the matter. If he is not neutral, he ought to be; for nothing in *That Noble Dream* supports the granting of positive value to autonomy—or negative value, either.

A story will perhaps help to link together these issues of synthesis and autonomy. The story is an encapsulated history of the enterprise of professional historiography. It is not the only story that can encapsulate that history, but it is, I think, an important one. In its broad outlines, my story goes, the history of professional historiography is closely connected to differing attitudes toward what we might call "the project of grand narrative." By "grand narrative," I mean the story that the world would tell if the world itself could tell its story.<sup>11</sup>

"In the beginning"—I mean, of course, in that benighted time before professional historiography existed—European intellectuals believed that there existed a grand narrative and that it was possible to tell that narrative now. More precisely, it was possible to retell the narrative, for the narrative in question was the story offered in Judaeo-Christian Scripture. Professional historians, with their commitment to *finding* the narrative, were unnecessary.

Somewhat later than the beginning, faith in the scriptural grand narrative diminished. Professional historians began to walk the earth. In the early phase of professional historiography, the dominant view was that there exists a grand narrative but that it cannot be told now: it can only be told in the future, after "further research" has been done. Such was Ranke's view—at least most of the time. As the late Leonard Krieger pointed out, this view kept Ranke's well-known concern with historical individualities anchored within the larger framework of universal history.<sup>12</sup> It was Lord Acton's view and J. B. Bury's as well. It was also, I suggest, the view of that vast majority of historians who never reflected on

<sup>9</sup> Novick, *That Noble Dream*, 361–411.

<sup>10</sup> Novick, *That Noble Dream*, 496.

<sup>11</sup> I borrow the term "grand narrative" from Jean-François Lyotard, although the definition offered here is my own. See Jean-François Lyotard, *The Postmodern Condition: A Report on Knowledge*, trans. Geoff Bennington and Brian Massumi, foreword by Fredric Jameson (Minneapolis, Minn., 1979; originally published in French in 1979), xxiii–xxiv and *passim*.

<sup>12</sup> Leonard Krieger, *Ranke: The Meaning of History* (Chicago, 1977), 100–04, 130–31, 160–63, 226–28, and elsewhere.

universal history but nonetheless wrote out of a fundamental faith in the validity of Western culture as they understood it.

In a later phase of professional historiography, after World War I, there was yet another change. Now historians became more distanced in their commitment to grand narrative. They continued to believe that there exists a grand narrative, but it was a peculiar grand narrative—a purely ideal narrative, a narrative that could never actually be told. Under this dispensation, “autonomy” and “synthesis” remained important values—positive terms in the professional historian’s lexicon, but no particular synthesis could ever win the approval of any more than a fraction of the profession.

Today, I would suggest, there are signs of a fourth phase or attitude. Novick’s book both describes the preconditions for a new attitude and, in part, exemplifies it. Whether that attitude comes to full blossom remains to be seen. In the fourth, “post-professional” phase, the dominant view would reject grand narrative entirely—but ironically (for an unironic rejection of grand narrative would end up reconstituting it in its pre-professional form). I imagine, here, historians who would no longer see terms like “synthesis” and “autonomy” as possessing positive value (but such terms would also not possess negative value). I imagine historians who would not in any way think that they were telling “the full story.” I imagine historians who could turn themselves into economists or philosophers or literary critics, and who could shift easily back and forth between such conflicting fields (for conflicting they most certainly are). I imagine historians who, at the same time, would be intellectuals, speaking within the field of historiography and outside it as well.

As someone who has seen, in more than one context, the erosion of previously unquestioned consensus, I find fragmentation to be in some ways profoundly disturbing. Yet, if the sociological transformation of the academy continues (and I am inclined to think that it will), consensus of the old sort will have a hard time surviving. In such a situation, unity on the substantive level—the unity provided by the telling of a single story—can only serve to exclude. Likewise, when disciplines become fragmented and when the cross-cuts between them begin to take on lives of their own, unity on the methodological level disappears. Perhaps the only way, finally, of holding together what once was seen (somewhat misleadingly) as a unified enterprise would be through sustained attention to the histories, sociologies, and rhetorics of historical study—that is, through examining precisely the diversities that have shadowed historiography from the beginning. In short, unity would come only at a reflective level—if it would come at all.

In the fourth phase, works like Novick’s, which fall within the hitherto professionally despised field of historiography or “historiology,” would assume an important integrative role. One can think, too, of other works of a similar kind, by such writers as R. G. Collingwood, Hayden White, Paul Veyne, and Louis Mink, which all offer a reflective glance at the historians’ project.<sup>13</sup> Yet consider the

<sup>13</sup> R. G. Collingwood, *The Idea of History* (Oxford, 1946), “Epilegomena,” 205–334; Hayden White, *Metahistory: The Historical Imagination in Nineteenth-Century Europe* (Baltimore, Md., 1973); Hayden White, *The Content of the Form: Narrative Discourse and Historical Representation* (Baltimore, 1987); Paul Veyne, *Writing History: Essay on Epistemology*, trans. Mina Moore-Rinvulcri (Middletown, Conn.,

character of the integration—for historians generally would be united only by a common recognition of the impossibility of their union (although historians specifically might be temporarily united in more substantive ways). The deep teaching of Novick's wise and learned book, it seems to me, is that integration, either substantive or methodological, is impossible, except by force or by forgetfulness, and in consequence is not to be desired.

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1984); and Louis O. Mink, *Historical Understanding*, Brian Fay, Eugene O. Golob, and Richard T. Vann, eds. (Ithaca, N.Y., 1987).

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*AHR Forum*  
My Correct Views on Everything

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PETER NOVICK

THERE IS NOTHING MORE TEDIOUS than the spectacle of disgruntled authors complaining that they have been misrepresented or, even worse, whimpering that they have been “misunderstood.” Academic authors, above all others, should be immunized from such concerns, after years of seeing the versions of our lectures we get back in blue books at the end of the term. Nevertheless, it seems to me worthwhile to take up a few misreadings, by the panelists and others, of what I wrote in *That Noble Dream*, since these misreadings can skew our understanding of “the objectivity question and the future of the historical profession.”

Linda Gordon repeatedly talks of my book as being organized around a “dichotomy” or “binary opposition” between two “positions”: objectivism and relativism. From the way she talks about relativism, it is clear that she thinks I wrote about—more important, that she believes there in fact *is*—some kind of relativist *methodological approach*, which I was describing (or perhaps promoting).

I’m not sure what Gordon means by historical relativism, but I know how I used the word in the book that is our point of departure. At the very beginning of *That Noble Dream*, I noted in passing that “relativism” and “relativist” were labels applied by defenders of the idea of objectivity to their critics—not self-designations—but that the labels stuck, and so I used them. “Relativism,” I wrote, “refers not to a positive position but rather to a critical stance vis-à-vis various elements in the objectivist synthesis, and, in general, doubts about the coherence of the notion of objectivity as applied to history.” I subsequently repeated this understanding of historical relativism on other occasions, and it informs the entire book.<sup>1</sup>

To the best of my knowledge, no American professional historian has ever advanced “relativism” as a doctrine, or as a method, or as anything other than a repudiation of the objectivist telos. Just as in matters religious, nonbelievers feel that they can get along without “god,” so we who are called relativists believe we can get along without “objectivity.” But just as fundamentalists construct the bogeyman of “secular humanism” as the alleged doctrine of nonbelievers, so we have the reification—and Gordon is by no means the worst offender here—of the “posture” or “doctrine” of relativism. Those of us who, like myself, get labeled as

I have borrowed (i.e., stolen) my title from Leszek Kolakowski, who once used it in a response to E. P. Thompson. I’ve always loved it, wanted to use it, and may never have another chance.

<sup>1</sup> Peter Novick, *That Noble Dream: The “Objectivity Question” and the American Historical Profession* (New York, 1988), 3.

extreme relativists share no doctrine. We have in common only the conviction that, as I wrote in my book, “to say of a work of history that it is or isn’t objective is to make an *empty* observation; to say something neither interesting nor useful.”<sup>2</sup> I have sometimes used the term “anti-objectivist” to describe so-called relativists, but that was, I think, a mistake. Although the term “a-objectivist” is clumsy and difficult to pronounce, it would, I think, be more accurate, by analogy with “amoralists” and “asexuals” who aren’t *against* morality or sexuality—just don’t think it very interesting, important, or relevant to their lives. (These are not, perhaps, the happiest examples, but the point is a purely formal, etymological one.)

Not just in principle but, with trivial exceptions, in practice, the historical scholarship that so-called relativists write is indistinguishable from that of their brothers and sisters who, in some sense or other, continue to “strive for objectivity.” If two historians, one a “nihilist” relativist and the other a dyed-in-the-wool objectivist, set out to produce a history of the Civil War, or a biography of George Washington, there is nothing about their “relativism” or “objectivism” per se that would lead them to do their research differently, frame their narrative or analysis differently, or, indeed, prevent their writing *identical* accounts.

What, then, is the “objectivity question” all about, since my critics and I don’t just disagree about the answer but about the question? I laid out my understanding of the question—in retrospect, perhaps not as forcefully as I might have—on the first and last pages of the introduction and repeated it in the very last paragraph of the book. (And, of course, the 600 plus pages in between were all based on this understanding.) The “objectivity question,” I wrote, has to do with “what professional historians are up to—what they think they are doing, or ought to be doing, when they write history”; with “the nature of our work . . . ways of thinking about the products we present . . . and the claims we make on behalf of those products.”<sup>3</sup> From the readers’ point of view, it’s a question of the sort of object they hold in their hands when they pick up a work of history. From the author’s standpoint, it’s a question of what sort of object is being placed in the mailbox when the manuscript is sent off.

To put it somewhat differently, the “objectivity question” is in no sense a methodological question, as Gordon and others would have it. It is only very ambiguously an epistemological question (which my dictionary defines as “pertaining to . . . the method or grounds of knowledge”). Rather, though I realize that the categories aren’t straightforward, it seems to me fundamentally an ontological question (having to do with, according to the same dictionary, “the being or essence of things”). The “things” in this case are both historical scholarship and the fruits of that scholarship. This is why the question is so highly charged: it goes not to the (methodological) issue of *how* we do our work but to *who* we are, *what* we’re doing, and what we’ve done when we’ve done it.

David Hollinger purports to see in my book an internal contradiction between “Postmodernist Theory and *Wissenschaftliche* Practice.” (Hollinger rips off the mask and reveals himself to be a deconstructionist.) Toward the end of his piece,

<sup>2</sup> Novick, *That Noble Dream*, 6.

<sup>3</sup> Novick, *That Noble Dream*, 1, 17, 628–29.



Hollinger specifies his dilemma. Even though I reject the objectivist myth, he says, I have written a book ideally suited to please those who defend the myth. (A bit of an overstatement—cf., e.g., Professor Hexter.) In what is a left-handed compliment if I ever saw one, Hollinger calls the book “a very traditional monograph” that is “attentive in the extreme to standard professional norms.” He goes on: “There may not be a paradox here, but if there is not, it would be good to have explained more fully why there is not. If an ‘argument’ can possess ‘relative autonomy . . . from details of the evidence’ as Novick tells us was true [of David Abraham’s book], why does Novick work so hard to document his own claims, rendering them less vulnerable to attack than were the claims of David Abraham?”<sup>4</sup>

Very much to the point here are some remarks by the philosopher of science Paul Feyerabend. And they are even more to the point if one reads what Feyerabend had to say about arguments as applying to mode of presentation as well. Feyerabend wrote: “An important rule of argumentation is that an argument does not reveal the ‘true beliefs’ of its author. *An argument is not a confession*, it is an instrument designed to make an opponent change his mind. The existence of arguments of a certain type in a book may permit the reader to infer what the author regards as effective persuasion, it does not permit him to infer what the author thinks is true.”<sup>5</sup>

I do indeed believe that an argument can possess “relative autonomy . . . from details of the evidence.” But most of my readers don’t share this belief—are in fact suspicious of any such claim. How do I win over those who can be won over and make difficulties for those who, if they could conveniently do so, would like to discredit my findings and conclusions by disparaging my scholarship? The question answers itself: by the most scrupulous adherence to *wissenschaftliche* (sometimes confused with “objectivist”) norms. If I were addressing a French audience, I’d speak French—and pay particular attention to my pronunciation, because you lose credibility with the French if you mangle their language. If, as the result of some revolution in historiographical sensibilities, the discipline demanded that findings be presented in sonnet form, I’d chop up what I had to say into fourteen-line chunks. Addressing the existing historical profession, which has its privileged idiom, its rules about what makes you gain credibility and what makes you lose it, its fetishized procedures and modes of discourse, I do those things that gain me credibility and avoid those things that would make me less believable and more vulnerable—that would embarrass and tend to discredit me. Those whose views are safely middle-of-the-road can risk carelessness; those of us

<sup>4</sup> David A. Hollinger, “Postmodernist Theory and *Wissenschaftliche* Practice,” *AHR*, 96 (June 1991): 691.

<sup>5</sup> Paul K. Feyerabend, *Science in a Free Society* (London, 1978), 156, italics in original. All of this is, of course, also relevant to Hollinger’s astonishing assertion—and not his alone—that my book presents no “argument.” Only those who have a very narrow (syllogistic) view of argumentation could say this. The entire book is an argument, which, by *redescribing* the objectivity question, attempts to get people to look at the question differently and perhaps, in the process, come to abandon what seem to me outworn shibboleths. I may have failed in this attempt, as I manifestly did with Hollinger, but that’s another matter. In any case, there are limits for what you can hope for in the case of a Haldeman or an Ehrlichman—a marvelous analogy, for which I am grateful to Hollinger, and which I wish I’d thought of when I was writing the book.

whose work in one way or another challenges conventional wisdom can't afford to, since what we write is bound to be scrutinized with more than ordinary care.

Let me now turn to a bit of scriptural exegesis, the scriptural text in question being, of course, "In those days there was no king in Israel; every man did that which was right in his own eyes." I should here apologize for somewhat imprecise imagery. The verse suggests a state of "individualistic" anarchy, while in fact, as I made clear at great length in the last section of the book, what we have today in the American historical profession is something akin to the Holy Roman Empire: 365 not-quite-sovereign but largely autonomous realms, with various systems of adjudication and of measurement, in ever-shifting alliances, ever amalgamating and bifurcating, linked by a progressively weakening allegiance to dimly remembered common values and ideals. Hardly anyone noticed the ultimate disappearance of the Holy Roman Empire, and only a handful of sentimental conservatives regretted its demise. All the worthwhile activities that men and women had conducted under the empire (and some not-so-worthwhile ones as well) were equally possible after this vestigial allegiance had disappeared. This qualification aside, I am flabbergasted that my characterization of the fragmented state of the discipline is regarded as "apocalyptic" by Hollinger and other critics. Of the panelists, Allan Megill alone sees that I in no way lament the state of affairs I described; that, like him, I see it instead as offering rich possibilities.

The widespread misreading of what I had to say on this matter illustrates the deplorable decay of scriptural literacy in this secular age. The verse describes a state of affairs not at "the end of days" but quite early in biblical history. Soon thereafter, the people of Israel told the prophet Samuel that they wanted "a king to lead us, such as all the other nations have." God instructed Samuel to "warn them solemnly and let them know what the king who will reign over them will do" (1 Samuel 8). The prophet warned them of the disasters that would follow; they wanted a king anyway; they got one, and the disasters followed in short order.

So long as we have no king—no coerced orthodoxy—I see no reason why we cannot peacefully coexist. If views differ on the ontology of historiography, everyday historical practice can continue undisturbed. Those who think as I do are content, in our historical work, to be suggestive, and we don't worry about being definitive. We want to offer what we hope will be fruitful—perhaps even "edifying"—new ways of looking at things in the past, without aspiring to any higher office. Others are, in a sense that seems to me deluded, but not pernicious, concerned with "moving toward the truth" or "getting it right." My friends and I can find the work of historians in the latter group suggestive and/or fruitful and/or edifying, while disregarding the far-reaching and, to us, irrelevant claims that are made for them. Conceivably, those in the other camp may conclude that we nihilists have sometimes—against our will, as it were—"moved toward the truth" or "gotten it right."

Gordon and some others who (like myself) think of themselves as being "on the left" find my plea for tolerant pluralism unsatisfactory. I am told that struggles within the academy are about *politics* and *power*; that by not focusing on these struggles, I am evading the main issue. Relations of power within the historical profession, as within all academic disciplines and institutions, are all too real. It is

precisely my awareness of configurations of power that convinces me that any call for a power struggle within the profession would be catastrophic for the left—however “left” is defined. I like to believe that even in a period of left ascendancy I would favor tolerant pluralism: it seems to me a value in its own right. But this principled resolve is unlikely to be tested in what gives every indication of being a very prolonged period of conservative dominion in American society and politics. We are beginning to see very serious conservative backlash in the academy—massively funded by right-wing foundations, cheered on by the mass media, and with increasing support from middle-of-the-road academics who will tolerate the occasional Marxist or feminist but who draw the line at those who would “undermine the foundations of Western rationality.” With a few well-placed and highly paid exceptions, the target of this onslaught, the historical left—especially the epistemological left—is weak and vulnerable, contrary to its own grandiose claims and the fevered fantasies of paranoid conservatives. In *this* power-political context, for dissidents of any kind to overreach themselves, and thus compromise that tolerant pluralism which (albeit imperfectly) provides a haven and base for heretics, seems to me *plus qu’un crime—une erreur*.

With all of its faults, the organized American historical profession, particularly in recent decades, has been the most ideologically open, the least exclusionary, of any such body in the world. And, within the American academic world, it continues to compare favorably in this respect to most other disciplines. It is when there is a king in Israel that I’ll be in need of Hexter’s advice to “cheer up.” Until then, concerning the historical profession if very little else, I’m quite cheerful enough.

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## *AHR Forum*

### Afterword

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DOROTHY ROSS

IN ORGANIZING THIS PANEL, I hoped that the participants would address the clouded future that Peter Novick leaves to the historical profession at the end of his book. New developments in philosophy, science, and the humanities, he said, have destroyed whatever credibility the ideal of objectivity once had. If knowledge is inevitably perspectival and uncertain, historians can only look to their linguistic and social practices to achieve intellectual authority and coherence in their work. Yet, just at this moment, the communal basis of those practices is disintegrating and the profession itself fragmenting. Attempts to find theoretical grounding for a more circumspect ideal of objectivity were not of much use, Novick implied, and, in any case, powerless to stop the centrifugal historical forces at work. Is Novick's a sound reading of our current situation? Whether or not he was lamenting such an outcome, should we?

The lively, overflowing audience that crowded into our room at the Annual Meeting suggests that these concerns are widely shared. At the risk of adding still another reading of our dilemma, let me suggest some points of agreement and disagreement among the panelists. There are two sets of issues involved in the discussion: first, what inferences should we draw from the theoretical critique of objectivity? Second, is the historical profession fragmenting, and how does that situation bear on the objectivity question?

J. H. Hexter is the only one among us who is willing simply to by-pass the philosophical critique of objectivity. He has long believed that it is irrelevant to writing good history. He bids us look closely not at Carl Becker the skeptic but at Becker the historical craftsman. We need only rely on the standards of craftsmanship, of good writing and "getting it right," which are taught and enforced by our scholarly community.<sup>1</sup>

The rest of us conclude that the philosophical issue cannot be so easily avoided and that the traditional ideal of objectivity is no longer credible. I *believe* that we accept Allan Megill's conclusion that neither singly nor together, now or in the future, in practice or ideal, can historians tell "the whole story." In different ways, however, Linda Gordon and David Hollinger suggest that it will be possible to

I want to thank Joan Williams for comments that helped to clarify my ideas—to the extent that they are clarified—on a number of key points.

<sup>1</sup> See J. H. Hexter, "The Historian and His Society: A Sociological Inquiry—Perhaps," in *Doing History* (Bloomington, Ind., 1971), esp. 96–97.

substitute for “the whole story” a lesser kind of “objectivity.” Gordon proposes we will find it by looking at what historians do rather than at what they say they do. I like her nice evocation of the experience of historical research with its quiet listening to voices from the past, but I do not think that experience yields something that can be called “objectivity” as opposed to “interpretation.” What she describes seems to me rather one stage *within* the hermeneutic process.

David Hollinger aligns himself with the effort to construct “hermeneutically self-conscious” redefinitions of the ideal of objectivity, specifically, redefinitions based on “communities of the competent” such as those proposed by Thomas Haskell and James T. Kloppenberg. Allan Megill is doubtful of this effort, and Peter Novick now unequivocally aligns himself with Megill. To understand the disagreement, we need to look more closely at the kind of redefinitions of objectivity Haskell and Kloppenberg have proposed.

Haskell offers a fine analysis of objectivity as impartiality, the willingness to take opponents’ arguments seriously into account, an analysis that allows both objectivity and political commitment.<sup>2</sup> Kloppenberg describes a process of confirmation/falsification of competing interpretations by reference to “hard data,” a problematic description but one that nonetheless tries to capture the fact that, for historians, evidence matters.<sup>3</sup> Hollinger points to similar professional norms when he praises Novick’s book for “a generous measure of critical detachment” and “empirical verification.” Indeed, Megill, too, in praising Novick for his “wide-ranging, ironic, dispassionate” account, accepts such limited meanings of objectivity. What then is the disagreement?

I think it is a difference of strategy and emphasis. Why do historians want to reformulate ideas of “objectivity”? My guess is they want to retain the connotations of the term that allow them to differentiate history from fiction—as in Gordon’s and Kloppenberg’s (different) emphases on evidence. Or that allow them to differentiate historiography from merely subjective opinion—as in Haskell and Hollinger’s “critical detachment.” Or that allow them to legitimate the particular kind of contextual and genetic analysis that historians do but that social scientists and literary critics do not do. These are, however, three different tasks. The old idea of objectivity as the whole story performed all three functions, but now we are better off addressing these tasks afresh. The term “objectivity” may be a liability rather than an asset in carrying them out.

One thing that redefinitions of historical objectivity based on intellectual communities cannot do, Megill wants to emphasize, is render historiography immune from the deeper forces of historical contingency at work in the profession. If knowledge is inescapably perspectival, all our questions, methods,

<sup>2</sup> Thomas L. Haskell, “Objectivity Is Not Neutrality: Rhetoric vs. Practice in Peter Novick’s *That Noble Dream*,” *History & Theory*, 29 (1990): esp. 129–36.

<sup>3</sup> James T. Kloppenberg, “Objectivity and Historicism: A Century of American Historical Writing,” *AHR*, 94 (October 1989): 1011–30. Given the fact that the Deweyan and Popperian language of verification and falsification by reference to facts is inadequate to explain the acceptance or rejection of paradigms in the natural sciences, it has to be much less adequate to explain the acceptance or rejection of historical interpretations, which are even more loosely tied to data and more deeply embedded in the changing configurations of history. Such terms do not seem to me to clarify the part evidence does play in the process and lend a specious aura of scientific certitude. See Thomas Kuhn, *The Structure of Scientific Revolutions*, 2d edn. (Chicago, 1970), chap. 12.

and standards of competence inescapably flow with the tides of history. I agree that only if we stand at the "center" of the profession do things look intact. "Fragmentation" is probably the wrong word to describe what is happening, because the multiplying perspectives often retain some links to each other. Fragmentation implies the breakup of a common core into particularist pieces, whereas the outcome is just as likely to be continuing, if shifting, affiliations around a distinctively historical practice.

Nonetheless, there are deep methodological divisions that have already opened in the profession and spawned partially independent communities. Novick's book was not awarded the prize of the Social Science History Association or of the American Studies Association. Critical perspectives from many quarters have called into question basic categories and values in our historiography. Indeed, it is in dealing with feminist and minority history that Novick's impartiality noticeably falters, as Gordon makes clear. Even more radically destabilizing are the perspectives from other disciplines, sealed off from us only by conventional practice yet offering conflicting visions of the approaches and standards appropriate to our problems. Our communities of the competent are within themselves and among themselves, a congeries of overlapping and disparate communities, which dispute—even if tacitly—standards of substance, method, and competence.

Although it is surely worthwhile to articulate professional norms such as impartiality and empirical grounding, which, despite this diversity, allow us to argue with and learn from each other, these norms do not reach the deeper problems of historical contingency. Also, they can slide over into efforts at professional containment. Haskell, for example, turns his standard of impartiality into a fixed basis for professional definition and hence into a tool for policing borders, for declaring certain intellectual values to be the essential determinants of the historians' community that must, in every instance, override other claims. He has been concerned with what he regards as the excessive influence of feminist advocacy on the profession, and such a standard, presumably, can separate legitimate women's history from improper feminist advocacy. But even Haskell's standard requires interpretation; it is no more immune from the contestations of history than any other, and it certainly functions for Haskell as part of a political program.<sup>4</sup>

Megill and Novick do not want any artificial containment of the deep imperative within historical contingency—the imperative to let people follow questions where they lead and enter into conversation with them. Here again, we would find Hollinger, Kloppenberg, and often Haskell in agreement, for they are among the ablest conversers in our profession and have been, first and foremost, the agents of perspectivalism in American historiography.<sup>5</sup> They are liberals but not saps. Liberals try to give us the best of both worlds, and at their best, precariously, they do so.

Novick not only allies himself with Megill but, in a curious way, he also stands

<sup>4</sup> Haskell, "Objectivity Is Not Neutrality," 143–52.

<sup>5</sup> David Hollinger's original discussion of the social basis of objectivity, for example, was part of an effort to open historians' minds to theoretical issues; "T. S. Kuhn's Theory of Science and Its Implications for History" (1973), *In the American Province: Studies in the History and Historiography of Ideas* (Baltimore, Md., 1985), 105–29.



close to Hexter. Novick seems to be saying that the philosophical critique of objectivity has no consequences for historians *qua* historians. It affects how we think about what we do, but it need not affect what we actually write. Those who accept objectivity and those who reject it can write "identical" histories. The belief in objectivity carries no bad consequences for the profession: objectivists are "deluded, but not pernicious."

This radical disjunction between epistemology and practice could be (in the vein of Megill's comment) a postmodern stance. That historians can go on as before without the illusion of an objective reality may simply confirm the strong critics of objectivity who argue that we never relied on that illusion anyway; it was always a construction based on our successful pragmatic judgments and coherences, and there is no reason for it to be missed. Or Novick's disjunction between theory and practice could be (in the vein of Hollinger's comment) the result of an atheoretical orientation typical of historians. The common coin of all the theoretical critiques of objectivity discussed in his book is the idea that all truth is perspectival, a view that leads to distinctive consequences about historical narrative, rhetoric, and the historically situated, dialogic position of the historian. It would be strange if the "historical sensibilities" of historians, as well as the way they "frame their narrative or analysis," were not affected. It seems strange, too, to discount such pernicious effects of objectivity on historical practice as "hyper-facticity."

That brings me to the great unfinished business of the forum: politics. Of all the factors that continually make and unmake agreement within and among the scholarly disciplines, politics may well be primary. J. H. Hexter indicated, lightly to be sure, that Novick's distress about the objectivity question was a product of the troubled waters of the 1960s. His own optimism, though it never flagged, now reflects the brightening History of Freedom: "Westward, look, the land is bright." For some Americans, the land always brightens in the west, even in Eastern Europe.

In the discussion following the panel, Joan Wallach Scott amplified Linda Gordon's comments and roundly criticized Novick for failing to face up to the implications of the strong political drive that his evidence showed to be at work beneath the discourse on objectivity. To accept a kind of "pluralism," Scott said, was not an answer but itself a tainted political position. In this reply, Novick seems to accept that political position on tactical grounds, and perhaps substantively as well. However, the issue is not the principle of free expression of views, which is unassailable, but the pretense that it is a sufficient guarantee of real freedom. The critique of "tolerant pluralism" from the left exposed the unequal distribution of power hidden in pluralism and disproved the claim that neutral processes and norms (like objectivity) provide adequate access to the historical conversation. To return now to the old regime of "tolerant pluralism" seems an inordinate counsel of despair and credits the old regime with gains that were made only, and can last only, by political struggle for true diversity.

Finally, we should notice that the political implications of Allan Megill's vision of future historical practice are not altogether clear. He called us to the interdisciplinary stance of the "intellectual" but did not locate that stance in

society. When Michael Roth asked how Megill's recommended posture of epistemological irony could be reconciled with political commitment, Hexter replied that the two need not be practiced simultaneously; Megill, that the two could, and often were in the profession, practiced simultaneously. If Megill's vision of history is "post-professional," it still employs the ironic trope that Hayden White has taught us to identify with the academic profession of history.

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## British Way, French Way: Opinion Building and Revolution in the Second French Slave Emancipation

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SEYMOUR DRESCHER

AFTER A GENERATION OF ENORMOUS SCHOLARLY FECUNDITY, interest in the history of the Atlantic slave system shows no signs of subsiding. It now constitutes one of the liveliest fields of comparative and interdisciplinary history. A recent flurry of centennial activities marking the history of abolition will probably continue unabated into bicentennial commemorations in 1991 of the St. Domingue slave uprising and the quincentenary in 1992 of Columbus's voyage.<sup>1</sup> This luxuriant centennialization is grounded in a distinctive set of historical benchmarks. Little more than two centuries ago, personal bondage was the prevailing form of labor in most of the world. Personal freedom, not slavery, was the peculiar institution. In 1772, Arthur Young estimated that only 33 million of the world's 775 million inhabitants could be called free. Adam Smith offered a similarly somber ratio to his students and prophesied that slavery was unlikely to disappear for ages, if ever.<sup>2</sup>

During the next century, a transformation occurred in the Atlantic slave labor system. It required profound changes in historical and moral frames of reference as well as in political and economic institutions. Once conceived primarily in terms of a Whiggish history of human progress, the transition entailed the development of complex and countervailing expansions of slavery and antislavery.<sup>3</sup>

I should like to thank Stanley L. Engerman and Daniel P. Resnick for their helpful comments.

<sup>1</sup> Beginning with the sesquicentennial commemoration of British slave emancipation at the University of Hull in 1983, a number of anniversaries have left collective volumes in their wakes. Of the four-volume series *Legacies of West Indian Slavery* (London, 1985), see especially *Out of Slavery*, Jack Hayward, ed.; and *Abolition and Its Aftermath*, David Richardson, ed.; also *Anuario de estudios americanos*, 43 (1986); *Estudios sobre la abolición de la esclavitud*, Francisco de Solano, ed. (Madrid, 1986); *British Capitalism and Caribbean Slavery: The Legacy of Eric Williams*, Barbara L. Solow and Stanley L. Engerman, eds. (Cambridge, 1987); *Hispanic American Historical Review*, 68, no. 3 (1988), anniversary issue on Brazilian emancipation, also published as Rebecca J. Scott, et al., *The Abolition of Slavery and the Aftermath of Emancipation in Brazil* (Durham, N.C., 1988); *The Meaning of Freedom: Economics, Politics and Culture after Slavery*, Frank McGlynn and Seymour Drescher, eds. (forthcoming); *The Atlantic Slave Trade: Who Gained and Who Lost?* J. E. Inikori and S. L. Engerman, eds. (forthcoming); and a number of forthcoming publications from Brazilian Centennial Conferences in 1988.

<sup>2</sup> Arthur Young, *Political Essays Concerning the Present State of the British Empire* (London, 1772), 20–21; Adam Smith, *Lectures on Jurisprudence*, R. L. Meek, et al., eds. (Oxford, 1978), 181, 186–87.

<sup>3</sup> See, among others, Seymour Drescher, *Econocide: British Slavery in the Era of Abolition* (Pittsburgh, 1977); David Brion Davis, *Slavery and Human Progress* (New York, 1984); and David Brion Davis, *The Problem of Slavery in Western Culture*, rev. edn. (New York, 1988), esp. 153–64; David Eltis, *Economic Growth and the Ending of the Transatlantic Slave Trade* (New York, 1987); Robin Blackburn, *The Overthrow of Colonial Slavery 1776–1848* (London, 1988); Robert W. Fogel, *Without Consent or Contract: The Rise and Fall of American Negro Slavery*, 4 vols. (New York, 1989– ), vol. 1.

From the limited defeat of slaveholders in England in 1772 through the passage of Brazil's "Golden Law" of emancipation in 1888, every seaboard society in the Atlantic world was affected by interdependent contractions and extensions of slavery. One Western polity after another eventually abolished its African slave trade. In the Danish and British dominions and the United States of America, the trade ended in the first decade of the nineteenth century. The Dutch, French, Brazilian, and Cuban slave trades closed in the second, fourth, sixth, and seventh decades of the century, respectively.<sup>4</sup> Chattel slavery itself began to be abolished in the non-plantation northern United States in the 1780s. French colonial slavery was temporarily (and in Haiti, permanently) abolished in the 1790s. On the Spanish-American mainland, the emancipation process, initiated during the wars of liberation, extended over half a century. British colonial emancipation came in the 1830s, and Swedish, Danish, and French emancipation followed in the 1840s. Slavery was ended in the U.S. South and the Dutch American colonies in the 1860s and in Cuba and Brazil in the 1880s.<sup>5</sup>

Although each of these processes has generated its own historiographical tradition, the most significant ones, quantitatively and qualitatively, deal with the American, British, and French cases.<sup>6</sup> In world-historical terms, British abolition is important both because of its predominant role in the ending of the Atlantic slave trade and because its slave emancipation became a stimulus and foil for other polities. Slaveholders, abolitionist societies, and political elites viewed British emancipation as the "great experiment" in the transition from slave to free labor.<sup>7</sup>

The history of British imperial slavery reflects the perception of British abolition as a long-term structural process. Historians now routinely discuss variables of demography, economics, political economy, ideology, class relationships, popular organization, and slave resistance in an international context. The principal historiographical issues revolve around the significance of countervailing tensions and ecological constraints on actions and outcomes.<sup>8</sup>

In contrast, French historiography has been dominated by the most successful

<sup>4</sup> Eltis, *Economic Growth*, 243–49, tables A.1–A.7 (imports of slaves into the Americas).

<sup>5</sup> On the general chronology, see C. Duncan Rice, *The Rise and Fall of Black Slavery* (London, 1975). For the period 1770–1823, see David Brion Davis, "A Calendar of Events. . .," *The Problem of Slavery in the Age of Revolution 1770–1823* (Ithaca, N.Y., 1975), 23–36. On Latin American chronology, see Herbert S. Klein, *African Slavery in Latin America and the Caribbean* (New York, 1986), chap. 11.

<sup>6</sup> See Joseph C. Miller, *Slavery: A Worldwide Bibliography 1900–1982* (White Plains, N.Y., 1985), and his updates in *Slavery and Abolition: A Journal of Comparative Studies*. This essay will deal with the British and French cases that have been historiographically designated as the exemplars or models for the abolitions of other polities.

<sup>7</sup> See William A. Green, *British Slave Emancipation: The Sugar Colonies and the Great Experiment, 1830–1865* (Oxford, 1976).

<sup>8</sup> See, among others, Eric Williams, *Capitalism and Slavery* (Chapel Hill, N.C., 1944); Roger T. Anstey, *The Atlantic Slave Trade and British Abolition, 1760–1810* (London, 1975), part 3; Howard Temperley, "Capitalism, Slavery and Ideology," *Past and Present*, 75 (1977): 44–118; Drescher, *Econocide*; Thomas L. Haskell, "Capitalism and the Origins of the Humanitarian Sensibility: Some Analytical Considerations," *AHR*, 90 (April and June 1985): 339–61 and 547–66; David Brion Davis, John Ashworth, and Thomas L. Haskell, *AHR Forum* on abolitionism, capitalism, and ideological hegemony, *AHR*, 92 (October 1987): 797–878; Eltis, *Economic Growth*; James Walvin, *England, Slaves and Freedom, 1776–1838* (London, 1986), part 2; Seymour Drescher, *Capitalism and Antislavery: British Mobilization in Comparative Perspective* (New York, 1987), chap. 5; *Slavery and British Society 1776–1846*, James Walvin, ed. (London, 1982); Solow and Engerman, eds., *British Capitalism and Caribbean Slavery*, part 3.

slave uprising in the Americas. For historians of slave resistance and of French slavery, the transformative power of revolutionary violence emerges as the pivotal moment in the history of Atlantic slavery. The first French emancipation decree in February 1794 was not only contingent on but was constrained by the forces of self-liberated West Indian slaves on the one hand and the impending British invasion on the other. The synergy of the French revolutionary ethos and the slave armies of St. Domingue are often treated as the turning points in the history of slave resistance and emancipation.<sup>9</sup>

Compared to British abolition, the most distinctive characteristic of the first French emancipation was the disintegration of metropolitan power over the course of events. From 1789, the contested question of policy toward slavery and the slave trade was inextricably entangled in struggles for colonial self-rule and racial equality. Planters' hopes for political autonomy and economic liberalization were combined with deep fears of metropolitan abolitionism and colonial free colored demands for civil rights. For three years, the colonial interest succeeded in keeping the slave trade and slavery off the agenda of the Constituent and Legislative assemblies despite the enormous social and ideological transformation within France.

Metropolitan antislavery was at best the concern of a small minority, increasingly distracted by bitter struggles over the direction and survival of the French Revolution. Among the revolutionaries themselves, bitter internal divisions tended to fragment perspectives on colonial developments. The disintegration of the political order and the economic system in the islands became a source of mutual accusation. Only a few months before the emancipation decree of 16 Pluviôse, An II (February 4, 1794), responsibility for the devastation of St. Domingue was still one of the counts of treason leveled against Brissot de Warville and other antislavery Girondists.<sup>10</sup>

<sup>9</sup> On the centrality of the Haitian revolution as a turning point in slave history, see C. L. R. James, *The Black Jacobins: Toussaint L'Ouverture and the San Domingo Revolution* (1938; rpt. edn., New York, 1963); Eugene D. Genovese, *From Rebellion to Revolution: Afro-American Slave Revolts in the Making of the New World* (New York, 1979), chap. 3; Elizabeth Fox-Genovese and E. D. Genovese, "Epilogue," *Fruits of Merchant Capital: Slavery and Bourgeois Property in the Rise and Expansion of Capitalism* (Oxford, 1983), 404–13. For a similar emphasis, see Blackburn, *Overthrow*, 30, 259–60. However, compare David Geggus, "The French and Haitian Revolutions and Resistance to Slavery in the Americas," *Revue française d'histoire d'outre-mer*, 76 (1989): 107–24. On the role of the Haitian Revolution in furthering or delaying the abolitionist process in Britain, see David Geggus, "British Opinion and the Emergence of Haiti 1791–1805," in Walvin, ed., *Slavery and British Society*, 123–49; and David Geggus, "Haiti and the Abolitionists: Opinion, Propaganda and International Politics in Britain and France, 1804–1838," in Richardson, ed., *Abolition and Its Aftermath*, 113–40. On the impact of British slave resistance in the abolition process, see Michael Craton, *Testing the Chains: Slave Rebellions in the British West Indies, 1629–1832* (Ithaca, N.Y., 1982). On the distinction between violence and nonviolence in the comparative history of slave emancipation, see, among others, Frank Tannenbaum, *Slave and Citizen: The Negro in the Americas* (New York, 1947), 105 and following; Eugene D. Genovese, *The World the Slaveholders Made: Two Essays in Interpretation* (1969; rpt. edn., Middletown, Mass., 1988), part 1; Seymour Drescher, "Two Variants of Antislavery: Religious Organization and Social Mobilization in Britain and France, 1780–1870," in *Anti-Slavery Religion and Reform: Essays in Memory of Roger Anstey*, Christine Bolt and Seymour Drescher, eds. (Folkestone, Kent, 1980), 43–63; Davis, *Slavery and Human Progress*, parts 2 and 3; Peter Kolchin, *Unfree Labor: American Slavery and Russian Serfdom* (Cambridge, Mass., 1987), epilogue; Seymour Drescher, "Brazilian Abolition in Comparative Perspective," *Hispanic American Historical Review*, 68 (1988): 429–60.

<sup>10</sup> See David Geggus, "Racial Equality, Slavery, and Colonial Secession during the Constituent Assembly," *AHR*, 94 (December 1989): 1290–1308; Yves Bénot, *La Révolution française et la fin des*

The fate of the first French slave system was determined more by the changing colonial and international situation than by metropolitan mobilization. The major juridical steps toward slave emancipation were decreed by Civil Commissioners in the colonies as emergency legislation. Although the ideological ground for 16 Pluviôse had been prepared by fiery public debates and pamphlet wars, emancipation in the colonies is best understood as a desperate response to wartime contingencies. In the wake of the revolution, this first emancipation remained, in the eyes of both metropolitan elites and later historical assessments, "The Formula for Loss of Control."<sup>11</sup>

Accounts of the second French slave emancipation, forty-six years after Napoleon's restoration of the institution, have remained heavily indebted to the revolutionary heritage. The abolition of 1848 superficially replicates the heroic and spasmodic French pattern of 1794. That the 1848 emancipation was punctuated by a slave uprising in Martinique, just before the arrival of the Parisian decree, reinforces the concept that the ending of the second French slavery derived from the same interaction of metropolitan and West Indian upheavals.

These twin scenarios of revolutionary transformation have reduced analytical incentives to investigate the historical context of the second French emancipation. There is a strong temptation to dismiss pre-1848 opinion building in a causal dissociation between July Monarchy procrastination, dominated by an inconclusive debate over the British model of emancipation, and the revolutionary efficacy of February 1848. As David Brion Davis has remarked, French emancipation "came in a reassuringly French way, at the hands of the left, led by [Victor] Schoelcher, an atheist untainted by British evangelicalism, echoing the events of the Revolution of 1789, and in response to both domestic and colonial turmoil."<sup>12</sup>

A somewhat different assessment of the second French emancipation has recently emerged. In a Marxist analysis, Robin Blackburn suggested a close analogy between French antislavery in the 1840s and British antislavery in the 1830s. During the late July Monarchy, the French mobilized against slavery in the same constellation of forces that impelled British parliamentary reform and slave emancipation. Louis Philippe's *juste milieu* echoed the context of late Hanoverian Britain. French antislavery on the eve of 1848 is conceived as the French analog

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*colonies* (Paris, 1988), chap. 7; Jean Tarrade, "Les Colonies et les principes de 1789: Les Assemblées révolutionnaires face au problème de l'esclavage," *Revue française d'histoire d'outre-mer*, 76 (1989): 9–34; and Blackburn, *Overthrow*, chap. 6.

<sup>11</sup> Davis, *Problem of Slavery in the Age of Revolution*, 137–48; also Geggus, "Racial Equality," 1304–05.

<sup>12</sup> Davis, *Slavery and Human Progress*, 284. For Davis, this is one of the principal ways in which British emancipation functioned as "A Deceptive Model" (224). On the dissociation between prerevolutionary abolitionism and emancipation, compare the successive volumes of the Cambridge History of Modern France: André Jardin and A.-J. Tudesq, *Restoration and Reaction, 1815–1848*, Elbourg Forster, trans. (Cambridge, 1983), 135; and Maurice Agulhon, *The Republic Experiment, 1849–1852*, Janet Lloyd, trans. (Cambridge, 1983), 27, 111. For a similar comparison of monarchical paralysis with republican revolutionary breakthrough, see Philippe Vigier's two "Que sais-je?" volumes: *La Monarchie de juillet* (Paris, 1962, 1982), 109–10, and *La Seconde République* (Paris, 1967), 24. See also A.-J. Tudesq, *Les Grands Notables en France (1840–1849): Etude historique d'une psychologie sociale*, 2 vols. (Paris, 1964), 2: 834–52, 1050. On slave resistance treated separately, see Edouard de Lépine, "Sur l'abolition de l'esclavage," in *Questions sur l'histoire antillaise* (Fort-de-France, 1978), 25–166; and Léo Elisabeth, *L'Abolition de l'esclavage à la Martinique* (Martinique, 1983), esp. 33–80.



to British antislavery, as part of a broad-based uprising against an oligarchic regime. Guizot is the French counterpart to William Pitt or George Canning, Louis Philippe to William IV. Paralleling the British agitation for metropolitan political reform, the abolition of slavery in 1848 was part of the resurgence of metropolitan agitation that led from the banquets of 1847 to the February Revolution. In the context of class polarization, the French antislavery campaign was “only a descant to this chorus of agitation, but both had a momentum dangerous for the prevailing order.”<sup>13</sup>

Blackburn, however, relied more on ideological inference than empirical reference. He posited an increasingly interlocking combination of ideologies and social movements, with massive working-class unrest, the mobilization of the frustrated “political classes,” and mass abolitionist agitation fusing into a mutually reinforcing juggernaut in February 1848. The bourgeois revolutionary leadership, aware of its historical obligation and “the pressure of a new type of class struggle,” had “to reassure the proletarian masses that its ideals were not to be degraded by the sordid interests of slaveholders.”<sup>14</sup>

An alternative picture of the relationship of prerevolutionary abolitionism to emancipation has been presented by Lawrence Jennings. In 1846–1847, some French abolitionists decided to adopt modes of agitation that had proven successful in Britain for more than half a century. Without an efficient religious and organizational network, the hastily composed French campaign “could not possibly succeed.” British-inspired efforts to persuade French departmental councils to call for an end to slavery had no “measurable effect on either these bodies or the Paris government.” The “apparent victory” of the British approach “was a shallow one.” French emancipation did not flow organically from a popular agitation, concluded Jennings, but coincidentally from the actions of an elite vanguard that displaced the *pays legal* notables. The decision to eradicate slavery in 1848 was more analogous to the Convention’s spasm of emancipationism in 1794 than to the long-agitated British emancipation of 1833.<sup>15</sup>

How are we to relate these two images of French abolitionism, the one popular, driven by class hostility, cumulative in impact, and analogous to its British exemplar in structure, with just a Gallic revolutionary touch, the other minuscule,

<sup>13</sup> Blackburn, *Overthrow*, 493–500. Few histories of the Revolution of 1848 devote more than the most cursory reference to the abolition of colonial slavery. See, for example, Georges Duveau, *1848: The Making of a Revolution*, Anne Carter, trans. (New York, 1967), 77. Where historians of France offer any causal statement, emancipation is the logical or “normal” consequence of universal male suffrage and the general humanitarianism of the Republicans of 1848. A minor ancillary theme may be a clash of economic interests—colonial cane versus metropolitan indigenous sugar-beet producers. See Agulhon, *Republican Experiment*, 27, 111; William B. Cohen, *The French Encounter with Africans: White Response to Blacks 1530–1880* (Bloomington, Ind., 1980), 192–93; Jean Dautry, *1848 et la II<sup>e</sup> République* (Paris, 1957), 101–02. In Gaston Martin’s *L’Abolition de l’esclavage* (Paris, 1948), the prerevolutionary petition campaigns serve to illustrate Victor Schoelcher’s prowess in converting the masses to his “Humanitarian Gospel” (41–42, 49–50).

<sup>14</sup> Blackburn, *Overthrow*, 494, 505–06. Blackburn’s scenario for 1848 is strikingly parallel to his account of the first metropolitan emancipation. The decree of 1794 arose in the context of “a subterranean current of rude popular egalitarianism,” anti-absolutist, anti-noblist, anti-capitalist, and proto-socialist. In both his narratives, the emancipatory moment becomes a revolutionary tableau, with all lines of action converging harmoniously on a central point (223–25, 261).

<sup>15</sup> Lawrence Jennings, *French Reaction to British Slave Emancipation* (Baton Rouge, La., 1988), chap. 7, esp. 185, 190, 193–94.

discontinuous, and reenacted in the incidental mode of the Great French Revolution? All recent accounts concur that one of the prevailing characteristics of organized French antislavery was its elitism. For most of its history between 1788 and 1848, French abolitionism was distinguished by its narrow political, social, and geographical base. It fit into what might be called the Continental, as distinguished from the Anglo-American, variant of antislavery. The prevailing features of Anglo-American antislavery were its relatively massive appeal, decentralized organization, and extended duration. Citizens in the United States and Great Britain attempted to develop associational mechanisms for bringing popular pressure to bear on hostile economic interests and hesitant agencies of the state. They used mass propaganda, petitions, newspapers, public meetings, lawsuits, boycotts, and electoral campaigns to develop an antislavery constituency. They aimed at inclusiveness of membership, welcoming participants who were otherwise excluded from the ordinary political process by reason of gender, religion, race, or class.<sup>16</sup>

French abolitionism, like most of its Continental counterparts, usually had very different characteristics. Its leaders were reluctant to seek mass recruitment. They preferred to work quietly on elaborate projects of abolition with the sanction of the state, often acting as brokers between external pressure groups (including British abolitionists) and their own slaveholding interests. Usually concentrated in the capital or chief commercial center of the nation, Continentals preferred to work in established governmental channels.

French antislavery, however, operated under peculiar historical conditions that made it somewhat anomalous within the "Continental" variety. In 1794, revolutionary France briefly incorporated slave liberation into a general program for revolutionary warfare. In 1802, Napoleon gave France the distinction of being the only Western nation to restore its overseas slave system. His restoration failed only in Haiti, where a brutal racial war resulted in the final expulsion of the French. Post-Napoleonic France was therefore heir to a complex history that linked French overseas initiatives to revolutionary violence, bloody restorations, military debacles, and colonial economic disaster. Postrevolutionary French abolitionism

<sup>16</sup> Drescher, *Capitalism and Antislavery*, chap. 3. Dale W. Tomich, in *Slavery in the Circuit of Sugar: Martinique and the World Economy, 1830–1848* (Baltimore, Md., 1990), 284–86, challenged the growing body of scholarly opinion that affirms a disjuncture between economic and political factors in the abolition process in the Americas, including Blackburn's treatment of French slave emancipation in 1848. Tomich charged that such scholarship isolates political and economic variables, emptying them of their historical context. His study, however, takes no account of French responses to the economic consequences of British slave emancipation, as discussed by Jennings. On the political side, Tomich's assertion that French slavery "was the object of mounting governmental and popular pressure throughout the period from 1830 to 1848" (p. 58) also overlooks a decade of historiography on the feebleness of such pressure during most of the July Monarchy. For further discussion of the historical debate over the role of capitalism in abolition, see David Brion Davis, "The Perils of Doing History by Ahistorical Abstraction: A Reply to Thomas L. Haskell's AHR Forum Reply," in Thomas Bender, ed., *The Antislavery Debate* (forthcoming, Berkeley, 1992), note 13: "Even scholars as far separated ideologically as Robin Blackburn and Robert William Fogel agree that the triumphs of abolitionism in Britain and the United States must be explained in political, not economic terms." If Davis sees a flaw in the explanations of Blackburn and Fogel, it derives from their relative insensitivity to "the central importance of religious motivation" (*ibid.*). Ms. kindly furnished by the author. In the French case, however, Davis himself does not consider religious motivation to have been of central importance in either of the slave emancipations. See Davis, *Slavery and Human Progress*, 284.

could not adopt the nationalist triumphalism so characteristic of its counterpart across the English Channel.

Even nonviolent mass agitation seemed threatening. Until the very eve of the February Revolution, abolitionists proposed plans of emancipation that were responsive to both slaveowner and conservative sensibilities. They made allowance for delays in implementation, for compensation funds, and for the continuity of plantation labor. Immediate and complete emancipation was not even considered as an abolitionist program until less than a year before the Revolution of 1848. The abolitionism that emerged during the Constitutional Monarchy (1815–1848) was as cautious in its recruitment as in its programs. With a limited membership, the movement was dominated by parliamentary and bureaucratic notables. Membership fees were high. Neither workers nor women were invited to join. One active abolitionist, Cyrille C. A. Bissette, was apparently kept at arm's length because he was an *homme de couleur*.<sup>17</sup>

French abolitionism was also "Continental" in being responsive primarily to exogenous (that is, British) initiatives. The impetus for the original "Amis de Noirs" in 1788 had been the formation of the London Abolitionist Society the year before. The first emancipation decree of 1794 came largely in response to a combination of unmanageable slave uprisings and British military threats in the Caribbean. Napoleon restored slavery immediately after the peace treaty of Amiens (1802), which assured the uninterrupted flow of his troops to the colonies. Napoleon's own decree of slave-trade abolition during his "Hundred Days" in 1815 was designed to forestall British hostilities. The revival of organized French abolitionism in 1834 was again a direct response to the implementation of British slave emancipation. The rationale for considering emancipation plans during the following decade stemmed from strategic concerns with potential "revolutionary" contagion from the British colonies and fears of British revolutionary warfare in the event of an Anglo-French conflict. Until the eve of the February Revolution, British policy was the principal exogenous stimulus to French action or inaction.<sup>18</sup>

THE NARROW CONFINES OF ABOLITIONIST MOBILIZATION for most of the July Monarchy set clear limits on the ideology and the pace of moves toward slave emancipation. On the one hand, a few eminent abolitionists made periodic motions in Parliament. The result was a sequence of decorous governmental investigations, piling up data and suggesting a range of legislative strategies. The abolitionists backed up their parliamentary activities with intermittent publications and articles in sympathetic Parisian newspapers. There was no strong link

<sup>17</sup> Drescher, "Two Variants of Anti-Slavery," 43–63; Seymour Drescher, *Dilemmas of Democracy: Tocqueville and Modernization* (Pittsburgh, 1968), 163. On Bissette's persecution in Martinique, which became a metropolitan *cause célèbre*, see Shelby McCloy, *The Negro in the French West Indies* (Lexington, Ky., 1966), 135–36. Bissette was listed as a corresponding member of the British Anti-Slavery Society (see *British and Foreign Anti-Slavery Reporter* [June 1, 1847]: 91).

<sup>18</sup> See Daniel P. Resnick, "The *Société des Amis des Noirs* and the Abolition of Slavery," *French Historical Studies*, 7 (1972): 558–69; Serge Daget, "A Model of the French Abolitionist Movement," in Bolt and Drescher, eds., *Anti-Slavery*, 64–79; Ruth F. Necheles, *The Abbé Grégoire 1788–1831: The Odyssey of an Egalitarian* (Westport, Conn., 1971), chaps. 10, 11; and Jennings, *French Reaction*, 201.

between individual abolitionist deputies and their local constituents, who were never informed of their representatives' abolitionist initiatives in electoral campaigns. Abolitionist parliamentary activity remained confined to prominent members in Paris.<sup>19</sup>

On the other side of the emancipation question lay the colonial interest. It was headed by paid delegates, who sat as members of the two parliamentary chambers by virtue of their metropolitan constituencies. The delegates of the four slave colonies (Bourbon, French Guiana, Guadeloupe, and Martinique) were in continuous contact with their respective colonial councils. They usually managed to present a united front on issues involving potential changes in the slave system. The colonials also had allies in the French provinces, especially among merchants in the Atlantic port cities. In this regard, the colonial interest was more national than that of the abolitionists. Through their delegate-legislators, the colonists possessed a lobby in the legislature and at the Ministry of the Navy and the Colonies.

The colonial interest also parried the propaganda thrusts of the abolitionist press. Given the restraints on political association, the press of the July Monarchy was the main channel of public opinion. The colonial delegates subsidized sympathetic editors through heavy subscriptions and direct grants to publicists. They compiled accounts of sums disbursed and assured their overseas paymasters that such subsidies ensured news and views favorable to the slaveholders' perspective. Occasional shifts of press support were carefully registered.<sup>20</sup>

The prominent French abolitionists wanted to avoid even the appearance of a radical or "fanatical" movement. They considered themselves engaged in a rational dialogue with the colonists and other metropolitan notables. They were committed to plans that ensured social order and sugar production during the transition from slavery to free labor. The idea of orderly and rational social change was embodied in a middle-of-the-road newspaper's declaration that French slavery would not be brought to an end by a revolutionary *journée* at the Bastille or at the Hôtel de Ville. The emancipation plan suggested by the French abolitionist members of the governmental commission of 1840–1842 allowed for a significant delay in implementation and constraints on the participation of slaves in the full system of metropolitan civil rights. French abolitionists anticipated the proletarianization of ex-slaves as the lasting solution to plantation labor problems.<sup>21</sup>

<sup>19</sup> See Tudesq, *Les Grands Notables*, 839–40; Drescher, *Capitalism*, 56–57, 203 n; and Jennings, *French Reaction*, chap. 4.

<sup>20</sup> See Tudesq, *Les Grands Notables*, 838–43; Lawrence Jennings, "La Presse havraise et l'esclavage," *Revue historique*, 272 (1984): 45–71. For details on press subsidies and sympathies, see Bibliothèque Nationale, Nouvelles Acquisitions, Fr. (hereafter, BN, Nouv. Acq.) 3631, Collection Schoelcher, Martinique, compte de M. Jollivet . . . (1842–48).

<sup>21</sup> See *Le Constitutionnel*, June 30, 1841, on the deprecation of revolutionary symbolism. The culminating example of social planning by notables for emancipation was the official report of the Duc de Broglie, *Commission instituée, par décision royale du 26 mai 1840, pour l'examen des questions relatives à l'esclavage et à la constitution publique des colonies: Procès-verbaux* (Paris, 1840–42); and *Rapport* (Paris, March 1843) (hereafter, Broglie Commission *Rapport*). See also Alexis de Tocqueville's parliamentary "Rapport sur l'abolition" (1839) and "Sur l'émancipation des esclaves" (1843), both published in the Gaillimard edition of his *Oeuvres*, Tome III, *Ecrits et discours politiques*, 3 vols. (Paris, 1962–90), 1:

If abolitionists did not generally accept the colonial hypothesis that slave property was as absolute and natural as any other, they agreed that slavery was a form of legal property nurtured by the metropolis. Planters who had invested capital in the security of French law were entitled to some indemnification for losses suffered through the eradication of that form of property. The language of property and the principle of no emancipation without indemnification was shared throughout the elite. In Parliament, abolitionists were reminded that the financial situation of the French government did not allow for an addition of 150 to 250 million francs to the national debt in order to provide compensation for slaveowners. New fiscal embarrassments, of course, continually succeeded old ones: the burdens of building the railroad system, of fortifying Paris, of conquering Algeria, of economic recession.<sup>22</sup>

A decade after British emancipation, there was a feeling of satisfaction on the pro-slavery side. The colonial delegates presented a united front against any major change. The colonial councils believed that their strategy of blanket resistance had proven successful against a variety of abolitionist proposals. The government was determined to reject any legislation that would incur a heavy financial cost. Baron Mackau, the minister of the navy and the colonies, was disposed to defend the colonial status quo. Metropolitan capital was again flowing outward to the colonies.<sup>23</sup>

By the mid-1840s, the French abolitionists themselves regarded the British precedent as a very mixed argument in favor of abolition. On the one hand, British emancipation had been completed without violence and with a rise in the ex-slaves' standard of living. On the other hand, there had been a decrease in plantation labor and output, counteracted only by an expensive policy of metropolitan protection for colonial sugar. As fears of revolution and war subsided in France, the economic difficulties of the British colonies seemed increasingly important. Abolitionists were aware that the momentum of the previous decade was slipping away from them.<sup>24</sup>

So apparent were the political constraints on abolitionist initiatives that André Jardin and A.-J. Tudesq used the politics of slave emancipation as the prime example of how the July Monarchy could be paralyzed by opposing pressure groups. A decade of French discussion produced only the "preparatory" Mackau Laws of 1845. These laws provided for little more than the slaves' right to purchase their own freedom and for increased religious instruction. According to Jardin and Tudesq, a pro-slavery minority had thwarted both governmental and public opinion favorable to emancipation by purchasing a sympathetic press.<sup>25</sup>

Given the constraints of indemnification and potential labor problems, more-

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41–111; and translated in *Tocqueville and Beaumont on Social Reform*, Seymour Drescher, ed. (New York, 1968), part 3, 98–173.

<sup>22</sup> Broglie Commission *Rapport*, 263–70; and Tudesq, *Les Grands Notables*, 847.

<sup>23</sup> BN, Nouv. Acq. 3631, Collection Schoelcher, report of Jollivet, Delegate for Martinique, March 13, 1843; May 13 and 28, 1843.

<sup>24</sup> Green, *British Slave Emancipation*, chap. 8. French abolitionist perspectives are detailed in Jennings, *French Reaction*, 103, 119, 178–80. For colonialist perspectives, see BN, Nouv. Acq. 3631, the reports of Baron Dupin and Jollivet, January 1845 to August 1846.

<sup>25</sup> Jardin and Tudesq, *Restoration and Reaction*, 135.



over, it is by no means clear that the government wished to move toward emancipation. The Guizot administration was insistent about the Gordian knot of public funding. Nor did any likely alternative administration promise a different policy. Adolphe Thiers, the principal leader of the opposition in the 1840s, was, if anything, more unsympathetic than Guizot to any imitation of the British experiment. In his view, the recently freed British colonies were a total failure, an example of the "base and barbarous idleness offered by Negroes left to their own devices."<sup>26</sup>

Regarding Jardin and Tudesq's observation about a pro-emancipation public, however, there is no evidence that opinion favored rapid legislative action. A decade after British emancipation, the French colonial delegates and the abolitionists agreed that the French public gave no indication that emancipation should be a political order of the day. Far more typical was the press's praise of the government's "sage lenteur" in approaching a complex problem. Guizot seemed more concerned with the public's hostility toward British activity against suspected French slavers than with French attitudes toward colonial slaveholders.<sup>27</sup>

Finally, as a result of the expanding French conquest of Algeria, there was far more territory with slaves under French sovereignty by the mid-1840s than there had been at the time of the July Revolution in 1830. In the name of internal security, General Bugeaud, the French military commander in Algeria, refused to take action against indigenous slaveholders. He even protested against interfering with the Arab slave trade, which he viewed as a principal means of expanding trade with the African interior. The frustrated tone of the British abolitionist press, the embarrassed correspondence of the French abolitionist society, and the complacent correspondence of the colonial delegates with their employers all testify to a regression from the modest abolitionist momentum of the 1830s.<sup>28</sup>

<sup>26</sup> Louis-Adolphe Thiers, *Histoire du Consulat et de l'Empire*, 21 vols. (Paris, 1845), 4: 176. In his history, Thiers was only repeating what he had said as president of the council of ministers in 1840.

<sup>27</sup> A. Cochut, "De la Société coloniale: Abolition de l'esclavage," *Revue des deux mondes*, 3 (1843): 177–228, 185–86. See Archives des Affaires Étrangères, Les États-Unis (Paris), 98 (1842), for Guizot's reference to A. F. de Bacourt (March 4), on the "exaggerated manifestations of public opinion" (Guizot deleted this phrase in the communication). In 1842, the one abolitionist attempt to hold an Anglo-French public meeting in Paris was prohibited by the government on the grounds that it would incite disorder. French planters used the absence of abolitionist public opinion in France as a strong argument against governmental action. See France, Ministère de la Marine et des Colonies, *Questions relatives à l'abolition de l'esclavage . . .* (Paris, 1840), Conseil Colonial de la Guadeloupe, session de 1840, 46.

<sup>28</sup> On abolitionist morale in the early 1840s, see Jennings, *French Reaction*, 170–80. Colonial expectations about the prospects of French slavery might also be measured in terms of price differentials between children and adults over time. A sharp relative drop in the former would have indicated a rising fear of proximate emancipation. However, calculations from Guadeloupe slave prices between 1825 and 1845 indicate that prices for children between ages one and thirteen declined 36 percent, only very slightly more than prices for slaves between fourteen and forty years old (33 percent). This does not mean that political events, especially British emancipation, had no impact on slave prices. In Guadeloupe, during the five years before British emancipation (1830–34), prices offered for seafaring slaves rose 4 percent over the previous period (1825–29), making them the only occupational group in 1830–34 whose prices did not decline. However, in the following five years (1835–39), immediately after British emancipation, this trend was reversed. Prices for seafaring slaves (with easy access to free islands) declined 37 percent, more rapidly than for those of any other group. The sources for these calculations are: France, Ministère de la Marine et des Colonies, *Questions relatives à l'abolition de l'esclavage* (Paris, 1843), part 3, *Délibérations et avis du Conseil spécial de la Guadeloupe*, "Tableau récapitulatif des prix des ventes d'esclaves . . . de 1825 à 1839"; "Résumé des



THE FIRST ATTEMPT TO IMITATE “British” methods of agitation came from outside the world of the notables under the sponsorship of a working-class newspaper. A few months after Guizot’s firm indication that the state of finances prohibited a decision on emancipation, the skilled artisans of Paris launched a drive for signatures. Petitions were initially gathered at printing shops. The Parisians were joined by workers of Lyons, who emphasized the sharper class consciousness of their signers by designating themselves “proletarians.” Although they evoked the example of their sans-culottes predecessors in the Great Revolution, the petitioners carefully emphasized the reformist intention of their campaign. They claimed the right to use the legal mechanisms of constitutional government for the pacific solution of slavery—for “the organization of labor for *all*,” and for “the emancipation of *all*.” The explicit model for such extraparliamentary mobilization was the “genius” of British petitioning. The workers noted that French rights of association and collective demonstration, still severely impeded by law and administrative surveillance, now had to be developed. The petitioners even accepted as valid the notables’ concern that slavery should not be followed “by vices and idleness” or by “pillage and murder—savage liberty.”<sup>29</sup>

Although it had the approval of individual members of the French Abolitionist Society, the petition was explicitly a workers’ artifact. The signers included carpenters, cabinetmakers, mechanics, clock makers, tailors, sculptors, jewelers, rope makers, foundry workers, laundresses, dressmakers, milliners, house painters, stoneworkers, sawyers, artists, and engravers. Both the Parisian “ouvriers” and the Lyons “prolétaires” welcomed female signatories. By Anglo-American standards, the results of the canvass were quite modest. Fewer than 9,000 signatures were presented to the Chamber of Deputies (7,126 from Paris and 1,704 from Lyons).<sup>30</sup>

To the colonial interest, the reception of the petitions was disturbing but not devastating. The petitions were the opportunity for antislavery speeches in the Chambers and for a renewed press discussion. But the petitioners, by virtue of their highly localized geographical and social origins, did not represent the limited electorate, the “legal country,” whose opinion counted most in the July Monarchy. As Thomas Jollivet, Martinique’s delegate, summarized it, the Chamber of Deputies, in forwarding the petitions to Minister Mackau, had reaffirmed its general approval of the principle of emancipation but not its immediate implementation. Jollivet concluded that both the Chambers and the public were

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transmissions de Noirs effectués à la Guadeloupe depuis 1825 jusqu’à 1839”; and Archives Nationales (AN), K3, Comité des Colonies (Juillet–Octobre 1848), Tableau A (revised), “Relève des ventes d’esclaves par catégories d’âge et par années.”

<sup>29</sup> See *L’Union*, *Bulletin des ouvriers, rédigé et publié par eux-mêmes* (February 1844), 2: 1–2. For similar remarks in the Parisian working-class press, see *L’Atelier* (May 1843): 66–67; (May 1844): 119. In the Chamber of Deputies on June 28, 1843, over the protests of abolitionists, Guizot estimated the cost of slave emancipation at from 220 to 250 million francs; see Drescher, *Tocqueville and Beaumont on Social Reform*, 172 and note.

<sup>30</sup> Drescher, *Capitalism*, 132–33. See also AN, Petitions to the Chamber of Deputies, C2425 (1844), 233.

still better disposed toward the colonies than they had been before the furor over the right-of-search controversy in 1841–1842.<sup>31</sup>

The only legislative sequel to the discussions of slavery in 1844 was the “Mackau Laws” of 1845. Given Mackau’s statement that no further “preparatory” legislation was contemplated, the colonial agents assumed that these laws were the limit of concessions to abolitionist demands. The workers’ petitions had not appreciably altered the prospects of French abolitionism, and a subsidized press could still be counted on to parry the abolitionist press. Early in 1846, the abolitionists were still referring to the “regression” of the emancipation question.<sup>32</sup>

At this juncture, the British Anti-Slavery Society played a major and perhaps crucial role in suggesting, exhorting, and supporting another petition campaign in 1846–1847. In 1844–1845, British emancipationists increased their missions to France. They expanded their geographical contacts in the hope of stimulating support from provinces outside Paris. They succeeded in convincing a Montauban professor, Guillaume de Félice, to compose an abolitionist pamphlet calling for a British-style popular movement and to organize provincial committees. They underwrote the distribution of Félice’s pamphlet and some of the expenses of Cyrille Bisette, the most active petition gatherer in Paris.<sup>33</sup>

Quantitatively, the results of the second campaign were hardly more impressive than the first. Just over 10,700 signatures were submitted to the two Chambers in the spring of 1847. Even at its relatively modest beginning, sixty years before, British popular abolitionism had outperformed the second French petition campaign. In 1788, the first abolitionist petition from Manchester alone, a city with fewer than 50,000 souls, produced the same number of the signatures as came from all of France sixty years later. By 1833, one man in five was signed up for immediate emancipation in Great Britain. Less than one in a thousand had yet done so in the French campaign of 1846–1847. Organizationally, the French abolitionist movement still remained divided. The elite abolitionist society would not even print the petitions for the 1846 campaign. In these respects, the diagnosis of July Monarchy abolitionism, as characterized by Jardin, Tudesq, and Jennings, is confirmed.<sup>34</sup>

Despite the quantitative contrast with its robust British counterpart, however, the French movement was clearly operating in a new way. In conscious imitation of the British model, the French religious network began to make itself accessible

<sup>31</sup> BN, Nouv. Acq. 3631, Collection Schoelcher, account of Delegate Jollivet, May 13, 1844.

<sup>32</sup> *Ibid.*, account of Martinique Delegate Baron Dupin, April 12, 1845, and Jollivet’s letter of September 28, 1845. For analogous assessments by the abolitionists, see Jennings, *French Reaction*, 188.

<sup>33</sup> See Guillaume de Félice, *Emancipation immédiate et complète des esclaves: Appel aux abolitionnistes* [sic] (Paris, 1846). See Jennings, *French Reaction*, 181–89, for an excellent discussion of the British role in the 1846 campaign.

<sup>34</sup> Seymour Drescher, “Public Opinion and the Destruction of British Slavery,” in *Slavery and British Society*, James Walvin, ed. (London, 1982), 22–48. Unfortunately, no historian has been able to locate the papers of the French petition campaigns. The network must be inferred from the Anglo-French correspondence in the Rhodes House Anti-Slavery Papers (Oxford), from notices of activities in British and French periodicals, and from the local elites who inscribed their names at the head of the lists of subscribers. Given the constraints of French association laws, potential signers could not be convened at publicly advertised meetings as they were in Great Britain or the United States. Abolitionist mobilization by mail and personal contact was again used by the French Protestant clergy in support of the North during the American Civil War.

to abolitionist action. French Protestants were already heavily overrepresented in the 1846–1847 campaign and promised to remain a major conduit for a new campaign in 1848. Even more striking was the entrance of the Catholic clergy and popular press into the antislavery movement. As late as 1845, Catholic officialdom assumed a general attitude of strict neutrality on the question of emancipation. In France, both the theological curriculum and the training of colonial clergy encouraged consideration of slavery as secondary to pastoral duties. When urged by British abolitionists to use its influence in favor of emancipation in 1842, the archdiocese of Paris responded by reiterating the need for preparing slaves for freedom through religious instruction.<sup>35</sup> A dramatic change occurred during the 1846–1847 campaign. Bissette took the emancipation petition to a large clerical retreat meeting in Paris. Hundreds of the Catholic clergy, including three bishops, signed up. A few months later, the archbishop of Paris seemed to remove impediments on further clerical participation. The liberal Abbé Dupanloup offered assurances that 20,000 clergy would sign an 1848 petition. The adherence of Louis Veuillot's *L'Univers* indicated that this figure was well within the power of Catholic mobilization. In the spring of 1847, his newspaper had managed to accumulate almost 90,000 pro-clerical adherents to a petition on educational legislation.<sup>36</sup>

By the fall of 1847, the third petition campaign was becoming more systematic than its predecessors, with the creation of a bureau of correspondence and the distribution of printed petition forms. Three petition-gathering agencies (Protestant, Catholic, and secular radical) were operating in addition to the elite abolitionist society. Frenchwomen were asked to sign their own petitions, following the precedent of their British counterparts a generation before. French abolitionism seemed poised to become a popular movement.

DID THIS NASCENT SOCIAL MOVEMENT have any appreciable effect in forwarding emancipation? Jennings argued no, citing the still obvious divisions in the movement, its lack of measurable impact in the provinces, and, above all, the government's refusal to take any major initiative between the presentation of the 1847 petitions and the overthrow of the regime the following year. David Brion Davis agreed that as late as 1847 there was no prospect of hastening slavery's demise. Blackburn, by contrast, emphasized the broad social aspirations, if not achievements, of the 1847 petition, the temporal coincidence between the banquets and the abolitionist campaign, and the fact that the February Revolution

<sup>35</sup> Drescher, "Two Variants," 51–52. For comparative analyses of Anglo-French missionary movements in relation to abolitionism, see also Bernard Salvaing, "Missionnaires catholiques françaises et protestants britanniques face à l'Afrique: Le Cas de la Côte du Benin et du Pays Yoruba (1841–1891)," *Revue française d'histoire d'outre-mer*, 71 (1984): 31–57; and Paule Brasseur, "Libermann et l'abolition de l'esclavage," *ibid.*, 73 (1986): 335–46. On the comparative impact of abolitionism on racism, see Seymour Drescher, "The Ending of the Slave Trade and the Evolution of European Scientific Racism," *Social Science History*, 14 (1990): 415–50.

<sup>36</sup> Drescher, "Two Variants," 51–52, 61 n.

subsequently empowered radicals who identified themselves with emancipation in the colonies.<sup>37</sup>

The alleged linkage between metropolitan French and colonial reformism is far weaker than Blackburn assumed. If one were to try and measure the salience of abolitionism on the basis of the public banquet campaign of 1847, the evidence is actually nil. Of the many issues evoked by the banqueters, colonial slavery was conspicuous by its absence. Just one week before the revolution, the deeply abolitionist *Le Semeur* lamented:

Hasn't the reader more than once observed that in the reformist banquets, where everything is discussed, our poor slaves haven't obtained the least space? The lot of the working classes in our country has elicited generous advocacy, and we are as happy about that as anyone. But the incomparably more abject condition of the blacks in our Antilles has been left in the shadows. Someone informed us that an honorable Deputy would speak in favor of the abolition of slavery at the Lille banquet, but we recall how this banquet was so torn apart by political disputes that the slaves lost the chance for the one mention that was to have been made of their miserable fate . . . When it moves beyond the Chambers to address the masses, the opposition is an echo, and when it ignores the cause of the slaves, it is because France itself is but feebly stirred.<sup>38</sup>

*Le Semeur* concluded that, contrary to their British forerunners, the reformers were not being pushed by popular interest to make even symbolic obeisance to the issue of emancipation. Blackburn's linkage of abolitionism to the metropolitan political revival of 1847 therefore remains stranded at the weakest level of causal analysis-temporal coincidence. The question of emancipation was no more prominent on the reformist agenda of 1847 than it had been in the *cahiers de doléances* of 1789.

A comparison between the petitions of 1844 and 1847 also shows that Blackburn's faith in the linkage of rising working-class militancy to rising abolitionist agitation is not borne out by social analysis. The petition of 1844 had been around 90 percent working class, its signers pointedly designating themselves as workers and proletarians. Although the majority of signers in 1847 were still residents of Paris and Lyons, the petitions had lost their working-class character. Those who headed up many of the petitions identified themselves as members of the elite: electors, mayors, magistrates, merchants, businessmen, clergy, lawyers, pharmacists, professors, and military officers.<sup>39</sup>

Yet there is abundant evidence that the campaign of 1846–1847 did have a measurable impact in France and indicated a renewal of abolitionist momentum on an unprecedented scale. Every year, the general councils of French departments could issue *vœux*, or resolutions, on current political or social issues. The

<sup>37</sup> Jennings, *French Reaction*, 191, 194; Davis, *Slavery and Human Progress*, 283; Blackburn, *Overthrow*, 494–95. Daget and Drescher concluded that, although the size and number of the French petitions is not evidence of a British-style popularity in French abolitionism, they do indicate the beginning of popular interest and an expression of solidarity by many workers of Paris and Lyons with the colonial slaves. See Daget, "Model," 74–75; Drescher, *Capitalism and Antislavery*, 132–33.

<sup>38</sup> *Le Semeur*, February 16, 1848.

<sup>39</sup> On slavery in the *cahiers* of 1789, see Beatrice F. Hyslop, *French Nationalism in 1789 according to the General Cahiers* (New York, 1934), 142; and Drescher, *Capitalism, and Antislavery*, 53–55. On the petitions of 1847, see Archives Nationales, Section Outre-Mer (hereafter, ANSOM), Aix-en-Provence, Généralités 197 (1489), 47 cahiers.

**TABLE**  
**Resolutions in Favor of Emancipation Presented by Conseils-Généraux of French Departments, 1835–1847**

<i>Date</i>	<i>Number</i>	<i>Number from New Departments</i>
1835	5	5
1836	6	1
1837	5	3
1838	12	3
1839	11	1
1840	6	1
1841	7	1
1842	4	0
1843	4	0
1844	3	0
1845	3	0
1846	4	1
1847*	24	16

\*Of the twenty-five councils that considered a motion on emancipation in 1847, one voted to table the motion, twelve supported immediate or very rapid emancipation, and twelve favored emancipation while insisting on suitable compensation, precautions, or guarantees of labor. This is a revision of the earlier breakdown of *voeux* in Drescher, *Capitalism and Antislavery*, 226.

SOURCE: ANSOM, *Généralités* 156 (1301), "Voeux des Conseils-Généraux pour l'abolition," 1835–1847.

table shows that there had been small surges of departmental resolutions in favor of emancipation following British slave emancipation (1834) and again following the ending of British colonial apprenticeship (1838). Conciliar *voeux* then dropped off in the early 1840s. The first "workers" petition of 1844 found no echo among the departmental notables. Then, in 1847, the number of abolition resolutions suddenly rose to twice the level of the previous peak years of 1838–1839. Sixteen departments took action on slavery for the first time. Never had more than a quarter of the nation's departments simultaneously taken a stand in favor of slave emancipation.<sup>40</sup>

The most immediate political effect of the petitions was registered beyond the metropolis. The government felt the need to anticipate the rising tide of abolition in some direction. During the previous parliamentary session, the two Chambers had received, and forwarded to the minister of war, petitions of notables requesting the abolition of slavery in Algeria. There the matter rested until April 24, 1847, when the popular petition was presented to acclaim in the Chamber of Deputies.

The same day, the minister of war dispatched a note to Marshal Bugeaud, the conqueror and governor-general of Algeria, indicating the government's need to bring the slave trade and slavery to a speedy end in the colony. The minister's

<sup>40</sup> Evidence for an "economic" sugar interest behind the departmental *voeux* on slavery is weak. Only one of the five major sugar-beet departments on the eve of 1848 was among those that continually voted for emancipation resolutions. Only one other "beet" department voted for such *voeux* more than once. On sugar-beet distribution, see E. Boizard and H. Tardieu, *Histoire de la législation des sucres, 1664–1891* (Paris, 1891).



haste was visible in the letter itself. Almost one-third of the original text (including a portion in which the minister undiplomatically confessed that he disagreed with the judgment of the abolitionists) was struck out and replaced by less self-revelatory wording.<sup>41</sup>

Although he acknowledged the difficulties that such a transformation would create for the colonial administration, the minister concluded that "the state of opinion" and British diplomatic successes in obtaining abolitionist promulgations in Tunis, Cairo, and Constantinople made it prudent for France to take preemptive action. Over the vitriolic opposition of the stunned and outraged governor-general, the minister dispatched the draft of an ordinance of emancipation on June 2. It was worded to prohibit immediately slave trading and slaveholding by Europeans and Algerian Jews and, in deference to Bugeaud's objections, to postpone abolition among the Muslim population to no later than 1852.<sup>42</sup>

The most important indicator of the petition's impact, however, can be observed in the reaction of the colonial delegates and colonists. In the months leading up to the presentation of the petitions, the delegates viewed the campaign with increasing anxiety. The convergence of press support for emancipation seemed particularly ominous when reinforced by a petition of 800 French priests meeting in Paris. Even before the petitions reached the Chambers, Delegate Jollivet sent word that "public opinion is turning against us." All the Parisian "organs of liberalism," from *Le Constitutionnel* to *La Réforme*, were "preaching emancipation." The press "anticipates our defeat and the Tribune [the Chambers] will complete it."<sup>43</sup>

The discussion of the petition in the Chamber of Deputies on April 24 and 26, 1847, was a disaster for the colonists. The cautious resistance of Minister Mackau was swept aside. Jollivet was driven from the rostrum in derision. Traditional defenders of the colonists on both the nationalist Left and the legitimist Right remained silent. Catholic leaders spoke in favor of the petition, and even the conservative Center, wrote Jollivet, "abandoned us." Martinique's humiliated delegate warned that abolition had just advanced by a decade. Within weeks, Mackau, abandoned by Guizot, resigned.<sup>44</sup>

If Jollivet's accounts of the situation decomposed into jeremiads after his personal humiliation, the assessment of Guadeloupe's delegate was even more somber. E. de Jabrun had been away from Paris during the debates and was not

<sup>41</sup> AN, F80 1699 (Algeria), "Minute" from the Minister of War, "Direction des affaires de l'Algérie, Bureau de l'administration générale et des affaires arabes, Projet de l'abolition de l'esclavage en Algérie," Paris, April 24, 1847 to (Marshal Bugeaud) the Duc d'Isly. Significantly, just a month before this dispatch, an internally written "Note pour le Ministre" (*ibid.*) emphasized the religious, political, and topographical obstacles to ending even the slave trade in Algeria.

<sup>42</sup> See BN, Nouv. Acq. 3631, Bugeaud's reply (May 2, 1847), and the Minister's second dispatch (June 2, 1847), enclosing a "Projet d'Ordonnance pour l'abolition de l'esclavage en Algérie." Further evidence of the campaign's impact may lie within the metropolitan strongholds of the colonial interest. Bordeaux's press had been overwhelmingly hostile to antislavery initiatives during the 1840s. In April and May of 1847, however, the normally antiabolitionist *Guienne* "unexpectedly opened its columns to a series of seven long articles containing passages highly critical of slavery." See L. C. Jennings, "Slavery and the Bordeaux Press," in *French History*, 3 (1989): 273–92.

<sup>43</sup> BN, Nouv. Acq. 3631, May 14, 1847.

<sup>44</sup> BN, Nouv. Acq. 3631, May 14, 1847.



personally devastated by the parliamentary *journées*. However, he witnessed the “movement of ideas” stirred up in the provinces by the Chamber’s emotional discussion and later reflected in the department resolutions: “All France would have applauded some kind of emancipation, had the Chamber been voting on it.”<sup>45</sup>

What struck the delegate most forcefully was that the indemnity question no longer offered unassailable security against serious legislation. A fundamental component of the “British” model of emancipation might itself be compromised if the government sought to ingratiate itself further with parliamentary abolitionists. Long-dismissed alternatives, such as the freeing of slave children at birth, or of slaves illegally imported after the abolition of the French slave trade, were again being mooted. Such piecemeal abolition would mean emancipation “at the cheapest possible price” for the legislators. The planters were facing the combined risk of reduced indemnification and decimated labor gangs.<sup>46</sup>

The colonists, hitherto united in opposition to emancipation, broke ranks. Some delegates urged a continuation of the old tactics and simply demanded more funds in order to counteract the abolitionist gains in the press. Others regarded the old strategy as bankrupt. Bourbon’s council failed to supply increased press funds. French Guiana was too poor to furnish further resources for a major counteroffensive in the metropolis. And Guadeloupe, responding to its delegate’s advice, passed over into the ranks of those favoring total and simultaneous emancipation.

Guadeloupe’s delegates and some colonial residents in Paris decided that the petitions were literally the handwriting on the wall. The next wave might inundate the whole system. Guadeloupe’s delegates therefore proposed a series of bold preventive measures. They first contacted the old eminent abolitionists, Broglie and Tocqueville, who had quietly withdrawn from abolitionist activity. The delegates also advised Guadeloupe’s council to do an about-face and to outflank all partial plans by a demand for total emancipation, the regulation of labor, and an indemnity.<sup>47</sup>

The distance traveled by some French slaveowners in 1847 was most startling at the ideological level. Until the mid-1840s, the French colonial interest aligned itself unambiguously with the sanctity of property. Colonists grounded their claims to indemnification on the absolute and natural right of property, as well as on French positive law. A regime that so clearly relied on degrees of property to distinguish a “legal country” of electables and voters from the vast mass of less privileged citizens was sensitive to such proprietary claims. The Guizot government’s main reason for not proceeding to emancipation was the necessity for fair

<sup>45</sup> BN, Nouv. Acq. 3631, E. de Jabrun, Delegate of Guadeloupe, letter to Martinique, September 12, 1847.

<sup>46</sup> BN, Nouv. Acq. 3631, Jabrun to General J.-J. Ambert, President of the Colonial Council of Guadeloupe, June 15, 1847.

<sup>47</sup> BN, Nouv. Acq. 3631, Jabrun to Martinique, September 12, 1847. In contemplating the future of their labor force, Guadeloupe’s planters had no incentive to invoke their own revolutionary past rather than the British model. The output of their major staple crops had declined by two-thirds or more during the revolutionary decade. See Christian Schnakenbourg, *Histoire de l’industrie sucrière en Guadeloupe aux XIX<sup>e</sup> et XX<sup>e</sup> siècles*: Tome I, *La Crise du système esclavagiste 1835–1847* (Paris, 1980), 122, 138.



"L'Abolition de l'esclavage à la Guadeloupe," d'après un document de la Bibliothèque Nationale, Jean Jaures, *Histoire socialiste: La Deuxième République française*, Vol. 9, *La République de 1848*, 349.

compensation, a precondition unchallenged by any potential heads of governments during the July Monarchy. Plantation slaveholders had envisioned transitions solely within the ambit of capitalism.

For more than a decade before 1847, however, French socialists had been suggesting that abolitionism was a false path to slave emancipation. Abolitionists looked no further than integrating colonial labor into the European capitalist model, producing emancipation-cum-proletarianization. Socialists with colonial links began to devise schemes to bypass laissez-faire abolitionism by an "organization of labor." This would give the colonial worker "social protection" and guarantee a labor supply to the employers in communities or corporations styled after Charles Fourier.<sup>48</sup>

The colonial planters and their delegates in Paris were aware of the socialist critique of abolition. Not one delegate or colonial council, however, so much as suggested a collectivist alternative to agents of government or to any of the various parliamentary commissions on French emancipation before the abolitionists turned to mass petitioning. Nevertheless, in the summer of 1847, many slaveholders in Paris and in Guadeloupe began to espouse the Fourierist language of labor. A majority of the Colonial Council of Guadeloupe adopted a plan of "association" to supersede slavery. The plan would avert the threat of piecemeal metropolitan emancipation by "full and indemnified" emancipation. Association would also presumably avoid the progressive disorganization of plantation labor by maintaining the plantation and sugar production as the foundation of the colonial economy. Finally, association might obviate certain difficulties encountered in Britain's colonies, where after emancipation the wages system had increased cash-flow problems and reduced plantation labor.<sup>49</sup>

The Guadeloupe plan did offer some resemblance to the Fourierist organization of labor. It was a sharecropping system, providing for a tripartite division of proceeds from production: one-third to labor, one-third to capital, and one-third

<sup>48</sup> As late as their participation in the proceedings of the Broglie Commission, not a single colonial council deviated from the proprietary defense, insisting on the indivisibility of property and denouncing "anti-social" doctrines. See ANSOM, *Généralités* 171 (1379), Report of the Committee of Guadeloupe's Council, charged with the examination of the Broglie *Rapport* (July 1843), 106: "Saint-Simonianism had its day . . . Abolitionism still pursues its course, more adroitly embedding itself among the powerful . . . All is mere vanity in the new socialist ideas, whether spread on one side of the ocean or the other." The willingness of Guadeloupe planters to consider sharecropping does not seem to be related to its decade of "free labor" in the revolutionary era. Concerning the organization of labor, Guadeloupe's council in 1840 declared slavery to be "in reality only the organization of the proletariat." Its own early experiment was conflated with Haiti's as proving the universal inferiority of free labor in the Caribbean. See France, Ministère de la Marine et des Colonies, *Questions relatives à l'abolition de l'esclavage: Avis des Conseils coloniaux* (Paris, 1840), 55–57; and ANSOM, *Généralités* 161 (1323), Observations of the Conseil des Délégués on Tocqueville's "Report on Abolition," December 20, 1839. French Guiana had a similarly negative attitude toward its episode of revolutionary emancipation. See *Avis des Conseils coloniaux*, Conseil Colonial de la Guyane Française (1840–41), 114.

<sup>49</sup> On the entry of socialist ideas into plans for colonial development and emancipation, see Jack Hayward, "From Utopian Socialism, via Abolitionism to the Colonisation of French Guiana: Jules Lechevalier's West Indian Fiascos, 1838–44," in Serge Daget, ed., *Colloque internationale sur la traite des noirs*, Nantes, 1985, 3 vols. (Nantes, 1988), vol. 1. My source is the unpublished version of 1985. The socialist critique of liberal abolition was evoked by each step in British emancipation. See Charles Fourier, "Aveuglement du libéralisme," *La Phalange*, 2, no. 28 (July 12, 1833): 331–32; and "Abolition de l'esclavage," 2, no. 46 (November 15, 1839): 785.

to "talent." It contained detailed rules for collective discipline, as well as welfare provisions for childhood, old age, and illness. But the plan stipulated that the shares for both managerial "talent" and for capital would accrue to the planters, who would retain authority over the new "organizations of labor." It also stipulated an initial five-year period during which the ex-slaves could not leave their new associations.<sup>50</sup>

The appeal of the Guadeloupe planters to metropolitan opinion was one of the most remarkable in the history of slavery. Nowhere else in the Americas did planters come so close to adopting the rhetoric of socialism in contemplating the transition to free labor, complete with a five-year labor plan. Where but in Paris on the eve of 1848 could slave planters have even imagined that such "advanced" rhetoric might gain them more sympathy than hostility?

Yet, during the final months of the July Monarchy, the impact of both the Guadeloupe plan and the abolitionist agitation seemed equally meager in terms of new legislation. Mackau's successor, the duc de Montebello, gave private assurances that the government was not preparing a major move toward emancipation. Indeed, he indicated a firm resolve not to be swept up in public opinion. When the Guadeloupe plan, accepting the principle of immediate emancipation, was presented as an address to the king, it was coolly received. The plan was declared too "advanced," and those colonial delegates who had rejected Guadeloupe's great leap forward felt vindicated in having stood on their traditional proprietary defense. At the opening of the Chambers session in 1848, the king's speech made no reference at all to colonial slavery.<sup>51</sup>

Martinique's council decided to cling to the slave system as long as the metropolis would allow it. Although slave discipline had deteriorated in response to metropolitan agitation, sugar exports from both Guadeloupe and Martinique in 1847 were up sharply over the previous year. Some of that increase might be attributed to the generally better harvest conditions of 1847, but, even so, the increase in the two French islands was clearly greater than it was in British Dominica, which lay between them. Moreover, in 1847, the British West Indies began to show the devastating effects of the dismantling of British imperial sugar protection. Bankruptcies were rampant, and the Jamaican and Barbadian banking systems were collapsing.

<sup>50</sup> ANSOM, *Généralités* 173 (1388), Guadeloupe's Projects of Emancipation, sent to the King, November 17, 1847. On the Fourierists' exhortations to the colonists in 1847, see the *Démocratie pacifique*, February 21, 1847. Its model for the organization of labor was a form of sharecropping based on an experiment in British Mauritius (*ibid.*, February 28).

<sup>51</sup> BN, Nouv. Acq. 3631, Dejean de la Batie to Martinique, August 10, 1847; Dupin to Martinique, February 9, 1848. Early in 1848, a document sent to the minister of the navy noted that "phalansteries" had been tried, without success, in France, England, Germany, and Africa. See ANSOM, *Généralités* 173 (1388), Paris, February 11, 1848. Apparently, not all of the colonists of Guadeloupe were happy about their delegate's initiative in the new organization of labor. The governor of Guadeloupe, M. J. F. Layrle, wrote to the minister of the navy on December 24 and 25, 1847, that the project of association was both languishing and suspect: "the colonists . . . have organized association as they would wish to organize servitude. Their project, such as it is, would be nothing but the continuation of slavery with an indemnity for the masters." Montebello, the new colonial minister, dismissed "association" as a dream. BN, Nouv. Acq. 3631, report of Jollivet, September 12, 1847. For abolitionist suspicion of the planters' conversion to association, see *Le Semeur*, February 9, 1848; and ANSOM, *Généralités* 162 (1324), Commission de l'Abolition de l'Esclavage, *Procès-verbaux*, 2d session, March 4, and *Annexes*, May 7.



Regarding Algeria, the minister of war was still assuring Guizot, in February 1848, that the emancipation ordinance drafted nine months before was receiving his closest attention. At the same time, he reminded his colleague, who had to respond to pressure from the British ambassador, that the situation was a delicate one. The Muslim population seemed to regard possession of slaves as a right. "The interests of domination enjoin us not to brusquely change a state of things which we implicitly promised to respect upon the capitulation of Algiers." Against the customs of the Muslim majority across the Mediterranean, Bugeaud's earlier dismissal of abolitionism in France retained residual weight. Where, Bugeaud had demanded to know, were the voices of millions of peasants and workers in France aligned with those of the liberal "théoriciens"?<sup>52</sup>

Yet the question of emancipation was no longer what it had been as late as 1844, when a small number of eminent abolitionists, colonists, and ministers engaged in a leisurely discourse with no real need to act. All of the protagonists were now keenly aware of the rising potential of pressure from without. If 10,000 petitioners could hasten the departure of a minister and increase the plausibility of "partial" emancipations, what might successive petitions of 100,000 or more not achieve? By the fall of 1847, the French abolitionists had settled into the perspective of British social movements. The government was henceforth to be assailed, year in and year out, by a rising tide of petition signers and propaganda. The political costs of defending the status quo increased. As one colonial delegate asked, how long would the government be willing to sacrifice its comfort for the sake of the colonists?<sup>53</sup>

Thus the abolitionists prepared to launch the "third wave" with enthusiasm. By the beginning of the 1848 session, they already claimed 30,000 signatures. The petitions that began to arrive in January of 1848 were from small provincial villages that had never before participated in abolition activity. For the first time, abolitionists were able to make use of the clerical networks that had been so useful to their British counterparts in the gathering of signatures. France was about to create a national movement at the village level, led by mayors, magistrates, priests, and pastors.<sup>54</sup>

<sup>52</sup> The annual production figures are taken from Noel Deerr, *The History of Sugar*, 2 vols. (London, 1949–50), 1: 235–42. For the plight of the British West Indies on the eve of the Revolution of 1848, see Green, *British Slave Emancipation*, 233–44; and ANSOM, *Généralités* 173 (1388), Address of Martinique's Council to the King, January 8, 1848. On governmental reluctance to implement emancipation in Algeria, see above, notes 40 and 41; and *ibid.*, the "Minute" to Foreign Minister Guizot, marked "Confidential," February 4, 1848. On Bugeaud's attitude, see his reply of May 2, 1847, and note 28 above.

<sup>53</sup> BN, Nouv. Acq. 3631, letter of Jabrun to General Ambert, June 15, 1847; and ANSOM, *Généralités* 173 (1388), letter of Reiset and Jabrun, June 29, 1847. See also Montebello's warning to Jollivet, just ten days before the February Revolution, that present legislation no longer sufficed for "the needs of the future." Jollivet's own registered assessment was that "all is finished," unless a full-scale press campaign could counter press hostility and the new petitions of the abolitionists; *ibid.*, reports of Jollivet, January 30, and February 14, 1848. In the Chamber of Peers, the liberal Catholic comte Arthur de Beugnot was preparing to launch the abolitionist drive of 1848.

<sup>54</sup> Symptomatic of the changing status of French abolitionism was the fact that when a new abolitionist movement was launched in Germany, in January 1848, it sent a birth notice to the secretary of the French Abolitionist Society. For the briefest of moments, Continental abolitionists no longer corresponded exclusively with and through London. See the copy of the *Aufruf zur bildung*

THE FEBRUARY REVOLUTION of 1848 overtook the tactics of abolitionists, colonists, and ministers alike. The Provisional Government, although dominated by a majority who had favored abolition before 1848, initially drew back from an immediate emancipation. On February 28, the first dispatch from Paris of the colonial delegates rejoiced that the Republic would, in the words of its own proclamation, organize "liberty in accord with the sacred principle of acquired rights and the interests of labor." Martinique's other delegate, Charles Dupin, was surprised and delighted to find himself in such harmony with François Arago, the new minister of the colonies. Dupin felt that he could move in perfect accord with the minister "until the elected national assembly will have legislated the terms of colonial liberty." The government was proceeding "under conservative principles."<sup>55</sup>

The situation changed dramatically on March 3. Victor Schoelcher returned from a voyage to Senegal. By 1848, Schoelcher had become France's most knowledgeable and indefatigable expert on slavery. He was the first and only French abolitionist to have visited the British, French, and Spanish Caribbean islands, Haiti, Egypt, and West Africa; he singlehandedly put abolition on the agenda of scientific congresses in France. Schoelcher was at the center of the petition campaign of 1847–1848 and produced a long, model petition, with advice on how to turn the operation into a full-blown British-style campaign. His affiliation with the political group associated with the newspaper *La Réforme* allowed him to conduct a continuous assault on the slave interest.<sup>56</sup>

Schoelcher's timely return to France was fortuitous. His advent to power was not, however, because the *Réforme* group had just entrenched itself in the new government. In a dramatic personal intervention, Schoelcher convinced Arago and the Provisional Government to prepare an emancipation decree in the shortest possible time. He was immediately chosen to preside over the emancipation committee. Within eight weeks, the emancipation decree was approved by the Provisional Government and published the day before the meeting of the National Assembly.<sup>57</sup>

Public opinion, as measured through the press, was not concerned with either emancipation or the colonies in February and early March of 1848. Nor does any account of the revolutionary crowds in Paris mention their having demanded

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eines Deutschen Nationalvereins für Abschaffung der Sklaverei, in the Château de Presles (papers of the Carnot family). The copy is addressed to Dutroné, the secretary of the French Abolitionist Society.

<sup>55</sup> BN, Nouv. Acq. 3631, dispatch of Dupin, February 28, 1848. The delegates of Guadeloupe and Bourbon suggested that the Republic adopt the association plan as the basis of emancipation. See George Boussenot, *L'Affaire légitime et la race noire* (Paris, 1912), 25. On the temporization of the Provisional Government's first colonial dispatch, see also "Note sur certaines épisodes de la Révolution de 1848 à la Martinique," by J. B. C. J. Colson, in Jacques Adélaïde-Merlande, ed., *Documents d'histoire antillaise et guyanaise 1814–1914* (n.p., 1979), 155–56.

<sup>56</sup> Among Victor Schoelcher's many writings, see especially *Colonies étrangères et Haiti, résultats de l'émancipation anglaise* (Paris, 1842–43); *Des colonies françaises: Abolition immédiate de l'esclavage* (Paris, 1842); *Histoire de l'esclavage pendant les deux dernières années* (Paris, 1847); *De la Pétition des ouvriers pour l'abolition immédiate de l'esclavage* (Paris, 1844), *Société française pour l'abolition de l'esclavage* (n.p., 1847). On Schoelcher's intervention at learned society meetings, see *Congrès scientifique de France 12<sup>e</sup> session tenue à Nîmes* (Nîmes, 1845), 312–13, 467–93.

<sup>57</sup> Janine Alexandre-Debray, *Victor Schoelcher, ou la mystique d'un athée* (Paris, 1980), 124–28. See also Louis Blanc, *1848: Historical Revelations* (1858; rpt. edn., New York, 1971), 267–69.



colonial slave emancipation. The revolutionaries at the Hôtel de Ville demanded immediate working-class representation in the government, a Republic, the right to work, and the red flag. Colonial slave emancipation was about as distant from the Parisian crowds in February of 1848 as it had been from the banquets a few months earlier. The Provisional Government therefore had a relatively free hand in formulating its pronouncements on colonial slavery, both in its cautious proclamation of February 28 and in its dramatic decision for immediate emancipation on March 4.<sup>58</sup>

Popular pressures for emancipation were as little evident among the colonial slaves as they were in metropolitan France. Prior to Schoelcher's return to Paris, the government received no news of colonial crowds like the ones in Paris that prompted the establishment of the national workshops. Indeed, before the revolution, there was no large-scale slave agitation, certainly nothing remotely like the "Baptist War" in Jamaica, less than two years before British emancipation. The first major French slave uprising of the decade came only in May 1848, in anticipation of the imminent Parisian emancipation decree.<sup>59</sup>

The initial cautious proclamation of February 28 therefore indicates the lack of a sense of emergency. Except for the organization of a Club des Amis des Noirs in Paris, the formation of Schoelcher's emancipation committee seems to have mobilized more notables in favor of postponement than of emancipation. A dozen provincial chambers of commerce petitioned the minister of the navy and the colonies for a delay. They and some colonial delegates requested that the decision be left to the forthcoming National Assembly.<sup>60</sup>

<sup>58</sup> Blackburn, *Overthrow*, 496. I have uncovered virtually no evidence of proletarian expectations regarding colonial labor in the spring of 1848, as posited by Blackburn (*Overthrow*, 506). As far as one can gather from the official proceedings, the Schoelcher commission was not subjected to mass abolitionist interventions. Neither the language nor the rituals of metropolitan labor were concerned with overseas slavery. Indeed, as Paris approached its peak of class polarization in June, Schoelcher was the target of a charge often aimed at British abolitionists, of narrowly focusing on slave liberation rather than the universal emancipation of labor (Etienne Cabet, in *Le Populaire*, June 4, 1848). For examples of the profoundly Franco and Eurocentric vision of 1848, see *Le Banquet social: Journal du XII<sup>e</sup> arrondissement*, April 25, 1848; and *Le Christ républicain*, June 18–21, 1848.

<sup>59</sup> Slave discipline deteriorated in the Antilles after news arrived of the Chamber of Deputies' response to the abolitionist petition in April 1847. But public disorders remained "extremely rare" (*La Réforme*, July 26, 1847). The first news of the colonial reaction to the February Revolution did not reach France until the end of April 1848. Before the decree was promulgated by the government, the colonies still appeared to be "tranquil," though in a state of expectant and "profound emotion." See *L'Assemblée nationale*, April 27, 1848; and *La Réforme*, April 26, 28, May 8, and June 15, 1848. On the discussion of Jamaica's "Baptist War" of 1831, see Drescher, *Capitalism and Antislavery*, 106–09, and 232 nn. 53–58. Metropolitan abolitionists viewed the emancipation process as complete by the beginning of May 1848. In a published letter "To my Overseas brothers, the Black Slaves and Mulattoes," D. Poléma, a "Negresse de la Martinique," urged her compatriots to return to work "after the battle" in imitation of "those to whom we owe our liberty"; *La Réforme*, May 2, 1848.

<sup>60</sup> See Drescher, *Capitalism*, 203, n. 21. On the radical Club des Amis des Noirs, see AN, C942, Enquête sur les événements de mai et juin 1848, Tome III, Pièces diverses, 4th dossier. The flurry of mercantile protests against immediate emancipation found little echo either in the press or in the electoral campaign for the National Assembly. The conservatives focused their propaganda on the national workshops, Louis Blanc, communism, socialism, and the surtax on land. Their defense of the "sacred interests" of religion, family, and property did not include a defense of property rights in persons. See Tudesq, *Les Grands Notables*, 2: 1050–51, 1063. For the counter-petitions, see ANSOM, Généralités 153 (1275). On the colonial lobby, see E. Tersen, "La Commission d'abolition de l'esclavage (4 mars–21 juillet 1848)," in *Actes du Congrès historique du centenaire de la Révolution de 1848* (Paris, 1948), 295–301. *La Réforme* was the most abolitionist of the Parisian dailies toward the

Schoelcher, however, as head of the emancipation committee, saw to it that the decree was edited, approved, and published before the convocation of the National Constituent Assembly. He feared awaiting consideration by the elected representatives. When questioned in July about his preemption of the nation's prerogative, he replied that if the emancipation committee had waited on the approval of the National Assembly, "slavery would still be in place in the territory of the French Republic." A National Assembly willing to risk a Parisian insurrection by closing down the costly national workshops would have hesitated to enact immediate slave emancipation-cum-indemnity. It might also have responded quite as ruthlessly to the slave insurrection in Martinique as it did to the working-class challenge in Paris.<sup>61</sup>

Public opinion did not propel Schoelcher toward immediate abolition. Nor did his prominence as the architect of slave emancipation assure him of electoral popularity in revolutionary Paris. When he ran for the Constituent Assembly in April 1848, he received only 6,000 Parisian votes, as against 260,000 for Lamartine and 105,000 for Lamennais, the least successful elected candidate. He ran far behind even such unsuccessful radical reformers as Raspail, Léroux, Considérant, and Cabet. Schoelcher was to play his major parliamentary role in the Second Republic as the representative of colonial ex-slaves.<sup>62</sup>

Nevertheless, Schoelcher had greater freedom of action in Paris during the spring of 1848 than any abolitionist in the history of slavery. The metropolitan conservatives were momentarily immobilized. The French planters, already dejected by the campaign of 1847, could not look abroad for succor as had their predecessors in 1793–1794. The Paris crowds made no colonial demands. The possible reaction of the slaves was a matter of speculation, but Schoelcher could reasonably claim to be forestalling just such a situation of bloody confrontation as that which faced the Convention in February 1794. Knowing that a decision on slavery was likely to precipitate a decision on compensation, Schoelcher initiated an emancipation process that turned out to be more like the nonviolent British transition of 1834 than the hecatombs of 1791–1804.

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end of the monarchy, devoting almost one column a week to slavery just before the revolution. After February, its colonial coverage almost vanished. Colonial correspondence, which arrived too late to influence the Schoelcher commission's deliberations, seemed to favor a rapid and definitive end to the postrevolutionary interregnum. See *La Réforme*, April 26, 28, May 8, June 15, and June 30, 1848. On the final flurry of lobbying before the publication of the decree, see *ibid.*, April 30–May 3, 1848.

<sup>61</sup> AN, K2, Compensation Commission, Procès-verbaux, 9<sup>e</sup> Séance, July 12, 1848. Members of the Club des Amis des Noirs were also fearful that the National Assembly might revoke the emancipation decree. (AN, C942, Enquête, dossier 4, Procès-verbaux, meeting of May 20.) The details of the Martinique slave uprising were reported to the National Assembly on June 22, the very eve of the climactic working-class uprising in Paris. The account occasioned an emotional reaction in the Assembly, including a denunciation of the Provisional Government for its precipitate promise of freedom. The French government had quelled similar resistance in 1831, stimulated by the July Revolution. See Léo Elisabeth, "La Domination française, de la paix d'Amiens à 1870," in Pierre Pluchon, ed., *Histoire des Antilles et de la Guyane* (Toulouse, 1982), 395.

<sup>62</sup> Alexandre-Debray, *Victor Schoelcher*, 140. Schoelcher fared no better during the supplementary Parisian elections to the National Assembly in June 1848. He was not supported by the electoral coalition of workers' corporations, national workshops, radical clubs and newspapers. See *L'Aimable faubourien*; *L'Apôtre du peuple*; *Les Bêtises de la semaine*; *Le Conspiration des poudres*; *L'Epoque*; *L'Organisation du travail*; *Le Populaire*; *Le Représentant du peuple*; *Le Travail*; *Le Travailleur par Mère Duchêne*; *La Vraie République* (issues of June 3–6, 1848).

WAS THE DECREE OF 1848 therefore as “totally extraneous” to July Monarchy abolitionism as the decree of 1794 was to the Amis des Noirs of 1789? The emancipation of 1848 was more deeply embedded in metropolitan political culture than was its predecessor. The Second Republic did not require four years to decide in favor of immediate emancipation. The gathering of over 10,000 names and the hearty reception of the petitions in 1847 publicly ratified the commitment of a significant segment of the monarchy’s political elite to immediate emancipation. Such prominent reformers were identified with colonial transformation in a manner beyond even the radical imagination of 1830 in the July Revolution.

There is another important sense in which the search for British analogs in French abolitionism is heuristically fruitful. When the February Revolution overthrew the monarchy, it also interrupted the development of a cross-class and ecumenical movement that had already put the previous regime under tangible pressure to enter into a transition to freedom, either along the lines of the British experiment of 1834–1838, or Denmark’s (British-inspired) legislation of 1847. By 1847, the French abolitionist movement was far more popular than its counterparts in Sweden, Denmark, or the Netherlands. The imminence of metropolitan emancipation was therefore more predictable in 1847 than it had been in 1793.<sup>63</sup>

Even without the 1848 Revolution, French slavery would have soon come to an end. Suppose a France that had somehow avoided the twin traumas of February 1848 and the Bonapartist coup of December 1851. Assume the continued growth of the abolitionist movement and of slave unruliness in raising the political and police costs of the status quo. Take account of the slowly shrinking slave population, the declining economic value of the French slave colonies, and the consequently ever-diminishing fiscal costs of indemnification.<sup>64</sup> Given this combination of actual and most probable trends, it is difficult to imagine any scenario in which French slavery could have survived the metropolitan economic boom of the 1850s. Finally, shame and pride also counted for something among a political elite imbued with the mid-century conception that their nation was falling behind in the great procession of human progress. Even the relatively minuscule mobilization of 1847 had caused Guizot to speculate privately that in 1848 he would introduce some legislation to “restore France to her moral level among the civilized nations.”<sup>65</sup>

<sup>63</sup> See Svend E. Green-Pedersen, “From Danish Abolition to Danish Emancipation: Some Considerations about the British versus the French Influence,” unpublished ms. kindly furnished by the author. In discussing French emancipation, Aimé Césaire reversed Karl Marx’s scathing comparison of 1848 with 1789. For Césaire, the “farce-mais grandiose” is the theatrical emancipation of 16 Pluviôse An II: “Slavery is abolished in ten minutes of embraces, fainting and tears.” It is the emancipation of April 1848 that is “sérieux.” Aimé Césaire, “Victor Schoelcher et l’abolition de l’esclavage,” in Victor Schoelcher, *Esclavage et Colonisation*, E. Tersen, ed. (Paris, 1948), 25–26. On the evanescence of 16 Pluviôse, see also Robert Stein, “The Revolution of 1789 and the Abolition of Slavery,” *Canadian Journal of History*, 17 (December 1982): 447–68.

<sup>64</sup> On long-term economic trends in the slave colonies, see Schnakenbourg, *Histoire*; and Tomich, *Slavery in the Circuit of Sugar*. Note that at the highest compensation rate suggested for masters by the Broglie Commission, the cost of indemnification to each person in France would have been one-seventh what it cost his or her British counterpart in 1833. See Drescher, “Public Opinion,” 45.

<sup>65</sup> Alexandre-Debray, *Victor Schoelcher*, 114. Guizot’s remark was made to the comte de Montalembert on August 12, 1847. In 1846–1847, the arsenal of shame expanded enormously for French abolitionists. Sweden legislated emancipation for its small slave colony of St. Bartholomew. Denmark enacted a program of gradual emancipation. The bey of Tunis decreed the end of slavery within his

It is difficult to conceive of nonrevolutionary conditions in which French slavery would have outlasted the sixth decade of the nineteenth century. Somewhere between the ten-year transition to emancipation suggested by the Broglie Commission's abolitionist majority in 1843 and the twenty years favored by the commission's pro-colonial minority, French slavery would probably have been terminated by the regular legislative process. Although the actual date of the second emancipation was fortuitous, and the implementation more rapid than in normal circumstances, French abolition was not an accident.<sup>66</sup>

Some of the benefits of France's mode of emancipation in 1848 seem clear. The slaves were spared another decade of existence as chattels and wageless laborers. For the first time in history, representatives of a black majority took their regularly elected places in a parliamentary democracy. But France's revolutionary acceleration came at a price. The newly freed blacks were exposed to the rapid retrenchment of their rights as citizens and workers as France again stumbled toward Bonapartist domination. A decade after 1848, French ships were again sailing from Africa with involuntary *engagés*, bought, "liberated," and dispatched to the colonies to work off the purchase price of their freedom. The only abolitionist movement on hand to oppose this traffic was located in Britain, where Victor Schoelcher fumed in exile. The "reassuringly French way" of revolutionary emancipation had also shattered the fragile coalition of liberals, radicals, church people, and workers that was forming on the eve of the February Revolution. Whatever the "deceptions" of the British model, French emancipation provided its own generous portion of shattered illusions for metropolitans and colonials alike.<sup>67</sup>

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jurisdiction. In Algiers itself, the Jewish congregation decided to prohibit slavery. The press did not fail to flaunt this flurry of antislavery among Protestants, Muslims, and Jews. Unaware of the government's draft ordinance for Algeria, *Le Semeur* asked whether France was content to have only Catholic powers unrepresented in the abolitionist ranks and to let non-Europeans outperform France in fostering progress; *Le Semeur*, November 26, 1846, May 20 and October 13, 1847; *L'Atelier*, 7 (April 1847): 487. For the impact of British abolitionism on Swedish emancipation, see Ernst Ekman, "Sweden, the Slave Trade and Slavery, 1784–1847," *Revue française d'histoire d'outre-mer*, 62 (1975): 221–31.

<sup>66</sup> French colonial postemancipation also deserves a larger niche in comparative studies. Eric Foner's stimulating *Nothing But Freedom: Emancipation and Its Legacy* (Baton Rouge, La., 1983), 3, assumes that the United States was "the only society where the freed slaves, within a few years of emancipation, enjoyed full political rights and a real measure of political power." Steven Hahn's wide-ranging comparative study of postemancipation societies also omits the French example of 1848; see "Class and State in Postemancipation Societies: Southern Planters in Comparative Perspective," *AHR*, 95 (February 1990): 75–98. By the time of U.S. emancipation, the French colonial freedmen had gone through a full cycle of political empowerment and political impotence.

<sup>67</sup> The steady erosion of abolitionism after 1848 is documented in Lawrence Jennings's account of official tolerance for French slaveowners in foreign countries after colonial emancipation; see "L'Abolition de l'esclavage par la II<sup>e</sup> République et ses effets en Louisiane 1848–1858," in *Revue française d'histoire d'outre-mer*, 56 (1969): 375–97. It was not only that Bonapartist imperial policy repeated itself in the Second Empire. Most ex-abolitionists in France seem to have greeted the erosion of freedmen's rights with the same "Gallic shrug of the shoulders" offered by most Jacobins when rights for colonial free blacks were revoked by the National Assembly in the fall of 1791. See Michael L. Kennedy, *The Jacobin Clubs in the French Revolution: The First Years* (Princeton, N.J., 1982), 209. On the significance of potential resistance by the freed population in preventing abrogation of certain articles of the emancipation decree after 1848, see ANSOM, *Généralités* 119 (1059), Decree of April 27, 1848, Modifications, 1848–1860; Pierre Lascade, *Esclavage et immigration: La Question de la main-d'œuvre aux Antilles* (Paris, 1907), 55–78.

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# The Voyage of the Vassals: Royal Power, Noble Obligations, and Merchant Capital before the Portuguese Restoration of Independence, 1624–1640

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STUART B. SCHWARTZ

Gluttons who are greedy of provinces, always die for want of digestion. No surfeit so dangerous as that of dominion.

Francisco de Quevedo, *Fortuna con Seso* (Zaragoza, 1650).

ON EASTER MORNING, April 1, 1625, with standards and pennants flying, their top decks draped in scarlet bunting, a magnificent fleet, the finest organized by Spain since the great Armada, crossed the bar of Saint Anthony and entered the Bay of All Saints on the Brazilian coast. It was a spectacular sight, the largest naval force ever to have crossed the Atlantic until then, its battle line extending over six leagues at sea; fifty-six ships, 1,185 guns, and 12,463 Castilian, Portuguese, and Neapolitan troops captained by the flower of the Iberian nobility and commanded by Don Fadrique de Toledo, Spain's ablest military leader.<sup>1</sup> The objective of this powerful fleet was the recapture of Salvador da Bahia, the capital of Portugal's Brazilian colony, from the Dutch, who had seized the city almost exactly one year before. Hopes ran high among the expeditionary force. The Spanish crown for once had mustered its might and its resources with alacrity and skill, giving lie to that old dictum, "help from Spain, late or never."<sup>2</sup> Within a month, the city had been liberated.

News of victory on this far-off tropical shore was received in Lisbon and Madrid

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<sup>1</sup> There are many accounts of the armada and the events of the fall and recapture of Salvador. The more-or-less official story is found in Tomás Tamayo y Vargas, *Restauración de la ciudad del Salvador y Bahía de Todos Santos* (Madrid, 1628); and Juan de Valencia y Guzmán, "Compendio historial de la jornada del Brasil y sucesos della," *Colección de documentos inéditos para la historia de España*, 55 (Madrid, 1870), 44–200. Valencia y Guzmán contains a number of tables listing the various contingents, but his addition is faulty, and thus the usually cited figure of 12,566 is in error. Malcolm Bochner, in his "Luso-Hispanic Relations in the Iberian Colonial World: 1580–1640" (unpublished typescript), brought the discrepancy to my attention.

<sup>2</sup> Among the fullest historical analyses of the fall and recapture of Bahia is still George Edmundson, "The Dutch Power in Brazil, 1624–54," *English Historical Review*, 11 (1896), 14 (1899), 15 (1900).



as a triumph of Catholic arms. In Lisbon, a great procession, artillery salutes, and fireworks signaled a jubilation only intensified when, later in the week, news arrived from Rome of the canonization of Queen Saint Isabel, “nossa Portuguesa.”<sup>3</sup> In both kingdoms, the victory was later celebrated in verse and commemorated in theatrical performances as well as a number of paintings, of which Juan Bautista Maino’s famous allegorical canvas and Lope de Vega’s lackluster play are the best known.<sup>4</sup> Chronicles and relations of the events were published in great numbers; many others were written by participants, observers, and historians.<sup>5</sup> No one who soldiered in the victorious troops ever failed to mention his service thereafter when seeking further reward or favor.<sup>6</sup>

This example of successful multinational cooperation especially impressed the count-duke of Olivares, Philip IV’s chief adviser and Spain’s most powerful minister. He commissioned Maino’s painting, in which he himself appears, to hang in the Hall of Realms at the Buen Retiro palace. Among the twelve victories commemorated there, five were from that *anno mirabilis* of 1625.<sup>7</sup> In his later justifications of an integrated Iberian monarchy under the leadership of Castile, the “Union of Arms,” the reconquest of Salvador always stood prominently as

<sup>3</sup> M. Lopes de Almeida, ed., *Memorial de Pero Roiz Soares* (Coimbra, 1953), 479.

<sup>4</sup> In verse, there is Gabriel de Calar, *Laurentina o poema épico de la victoria que tuvo contra los holandeses* (Salvador, 1985). In addition to Lope de Vega’s *El Brasil restituído*, there is also Juan Antonio Correa, *Perdida y restauración de la Baía de Todos Santos*. See J. Carlos Lisboa, *Uma Peça desconhecida sobre os holandeses na Bahia* (Rio de Janeiro, 1961). On Lope de Vega’s play, see José Maria Viqueira Barreiro, *El Lusitanismo de Lope de Vega y su comédia “El Brasil Restituído”* (Coimbra, 1950).

<sup>5</sup> The literature on the fall and recapture of Salvador is enormous, as C. R. Boxer pointed out in his excellent and succinct description of events in *Salvador de Sá and the Struggle for Brazil and Angola* (London, 1952), 40–68. José Honório Rodrigues, *Historiografia e bibliografia do domínio holandês no Brasil* (Rio de Janeiro, 1949), lists sixty coeval or contemporary items, but this is not complete. In addition to the main official and semiofficial chronicles such as Fadrique de Toledo’s own *Relación del suceso del armada y ejército que fue al socorro del Brasil* (Madrid, 1625), there is also an extensive pamphlet literature in both Spanish and Portuguese written by participants and nonparticipants. See, for example, João Medeiros Correia, *Relaçam verdadeira de tudo o succedido na restauração da Bahia* (Lisbon, 1625); Jacinto Aguilar y Prado, *Escrito histórico de la insigne y baliente jornada del Brasil* (Madrid, 1625); Bartolomé Rodrigues de Burgos, *Relación de la jornada del Brasil escrita a Juan de Castro escriviano publico de Cadiz por . . . escriviano mayor de la armada* (Cadiz, 1625). The Neopolitan role has not received adequate study, but a beginning is provided in Gino Doria, “I soldati napoletani nelle guerre del Brasile contro gli olandesi,” *Archivio storico per le province Napoletane*, n.s., 55 (1932): 224–50. In addition to the published accounts are many that remained in manuscript. See, for example, “Expediitio Brasilica,” por Franciscus de Macedo, 36 fols., Real Academia de Historia de Madrid, 9–31–8–7119.

<sup>6</sup> Over the years, I have seen hundreds of petitions (Spanish *servicios y méritos* and Portuguese *requerimentos*) scattered in many archives in which previous service in the Bahian expedition is prominently noted. See, for example, petition of Gabriel de Almeida, who served in the voyage and later with Oquendo’s fleet, Archivo General de Simancas (hereafter, AGS), Secretarias provinciales 1528, fol. 9; or that of Pascoal Teixeira Pinto, who served in an infantry company with considerable “spirit,” Arquivo Histórico Ultramarino (hereafter, AHU), Bahia papéis avulsos 4, February 8, 1644. Even the clerics took special pride in their service at Bahia; see the petition of the Benedictine priest Antônio de Jesu, who had served as chaplain under General Manuel de Menezes and was later with Don Fadrique when he surprised the English at Cadiz; AHU, Bahia papéis avulsos caixa 2, August 21, 1635. Widows and children of participants also pointed with pride to this service long after the event.

<sup>7</sup> Jonathan Brown and John H. Elliott, *A Palace for the King: The Buen Retiro and the Court of Philip IV* (New Haven, Conn., 1980), 162–64, 184–90. See also Enrique Marco Dorta, *La Recuperación de Bahia por Don Fadrique de Toledo (1625): Un Cuadro español de época* (Seville, 1959).



proof of what could be accomplished.<sup>8</sup> But neither Olivares nor anyone else at the time realized that the Bahian expedition marked a turning point in the history of the Iberian Atlantic, a last enactment of the old ties between the nobility and the crown, and a theater in which the roles of a unifying state, a mercantile bourgeoisie of suspect orthodoxy, and an increasingly disenchanted Portuguese nobility were played out. Within fifteen years, the dream of an integrated Iberian monarchy and a unified empire was in shambles, and Portugal had embarked on an independent course, an event that contributed “more than any other single factor to the dissolution of Spanish hegemony.”<sup>9</sup>

I WISH TO FOLLOW THREE THREADS that predominate in the reports of the loss and recovery of Bahia in 1625 and show how their representation in those accounts symbolizes the struggle between certain social groups in Iberian society and the relationship of those groups to the state. The complex web of politics, economics, and religion that eventually produced the Portuguese rebellion of 1640, and continued to dominate the life of that country for a century thereafter, was previewed in the Bahian episode. The three themes central to the accounts of the loss and reconquest of Salvador are the enthusiastic participation and valor of the Portuguese nobility in the reconquest of the city; the perfidious treason of the Jews, or, more exactly, of the “New Christians,” the descendants of converted Jews, in the city’s fall; and the advantages of Spanish and Portuguese cooperation in the face of a common enemy. The roles and status of the nobility and the New Christians (and, by extension, the commercial bourgeoisie) and their relation to royal authority in both Spain and Portugal are central to understanding the process that eventually divided the two Iberian empires, previously united under Habsburg rule since Philip II of Spain inherited, bought, and conquered the neighboring kingdom in 1580.<sup>10</sup>

The positions of nobles and merchants in relation to the growth of state power is, of course, at the core of discussion about the nature of societies and politics throughout Europe in the early modern period.<sup>11</sup> In the Portuguese case, the

<sup>8</sup> See, for example, Olivares’s reference to the defense of Bahia in John H. Elliott and José F. de la Peña, eds., *Memoriales y cartas del Conde Duque de Olivares*, 2 vols. (Madrid, 1978), 1: 185.

<sup>9</sup> R. A. Stradling, in *Philip IV and the Government of Spain, 1621–1665* (Cambridge, 1988), 298, stated: “In every conceivable dimension of the subject, it is hardly possible to exaggerate the significance of the Portuguese rebellion in the collapse of the Spanish system.” He made the same point in *Europe and the Decline of Spain* (London, 1981), 107, where he saw Portuguese anti-Semitism and anti-Castilian attitudes linked in the rebellion.

<sup>10</sup> Because of familiarity for most English-speaking readers, I have used the Spanish rather than the Portuguese numeration. Philip II, III, and IV of Spain were Philip I, II, III of Portugal.

<sup>11</sup> The literature on this theme is overwhelming. Perry Anderson, *Lineages of the Absolutist State* (London, 1974), is a starting point for much of the discussion. Theodore Rabb, *The Struggle for Stability in Early Modern Europe* (New York, 1975), provides a broad perspective that integrates cultural and political phenomena. I have found Niels Steensgaard, “The Seventeenth Century Crisis,” in Geoffrey Parker and Lesley M. Smith, eds., *The General Crisis of the Seventeenth Century* (London, 1978), 26–56, to be helpful on economic aspects; and Ralph E. Giesey, “State-Building in Early Modern France: The Role of Royal Officialdom,” *Journal of Modern History*, 55 (1983): 191–207, provides a perceptive discussion of bureaucracy in the state. For the Portuguese case, Vitorino Magalhães Godinho, *A Estrutura na antiga sociedade portuguesa* (Lisbon, 1971), is a good analytical

mediating themes of “national” independence, religious antagonism, and imperial decline complicate these more general issues. Seventeenth-century Portugal presents a precociously centralized state, metropole of a vast maritime empire, dependent on the ideology and support of a traditional social order but faced with an economic situation that could seemingly only be resolved by upsetting that order. While in most societies of that time it is too crude to speak of a class struggle between the nobility and the bourgeoisie because of predominance of aristocratic values at all social levels, in the Portuguese case the antagonism is sharper and more focused because of the religious-ethnic question.<sup>12</sup> The manner in which the crown eventually resolved—or failed to resolve—the anomaly between its social base and its economic needs determined much of Portugal’s subsequent history.

The fall and recapture of Salvador can be briefly summarized. The Dutch West India Company, formed in June 1621, sought a profitable and convenient way to wage war against Spain. Salvador, because of its location and sugar industry, offered a suitable target, and the Dutch hoped that the Portuguese dislike of Spanish rule might even cause the defenders to welcome the Dutch arrival.<sup>13</sup> A powerful fleet was organized in 1623, but Spanish agents learned of its objective, and warnings were sent to the governor at Salvador. His efforts to prepare a defense were frustrated by local planters and the bishop, who were unconvinced of the danger. The Dutch fleet arrived on May 9, 1624. After a day of indecisive bombardment, the bravery of Vice-Admiral Piet Heyn (of later silver-fleet fame) in silencing the Portuguese shore batteries and the lackluster performance of the local Brazilian troops resulted in the city’s fall. Panic set in after the first day’s hostilities, and the Portuguese abandoned the city, which along with the governor and a few loyal companions was captured when the Dutch marched in without resistance on the following day.

The Dutch success was short-lived. In the countryside, the Portuguese soon regained their composure, and an active guerilla campaign directed by the now-bellicose bishop kept the Dutch pinned down in the city during the following year. Meanwhile, the Spanish crown mobilized its response. The Spanish and Portuguese relief fleets met at Cape Verde and sailed for Bahia in February. The siege began on April 1, 1625, and, after some spirited initial fighting, the Dutch became demoralized, surrendering on May 1, 1625, to generous terms.

Literary celebration of the victory ranged from eyewitness accounts by commanders and common soldiers to official chronicles from both the Spanish and the Portuguese contingents. The Spanish royal chronicler, Tomás Tamayo de Vargas, under royal direction and with access to various sources, produced the official history in 1628, but other histories, like that of participant Juan de Valencia y Guzmán or historian, later to be bishop of Toledo, Eugenio de

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starting point that should now be used along with the exhaustive A. M. Hespanha, *As Vésperas do Leviathan: Instituições e poder político, Portugal, século XVII*, 2 vols. ([Lisbon?] 1986).

<sup>12</sup> Perez Zagorin, *Rebels and Rulers 1500–1660*, 2 vols. (Cambridge, 1982), 1: 71–77. Zagorin criticized a number of authors who, like Roland Mousnier, see class conflict in the social turmoil of the period. Zagorin’s point that the bourgeoisie hoped to acquire noble status is well taken but does not necessarily preclude class conflict or prove his case.

<sup>13</sup> Jonathan I. Israel, *The Dutch Republic and the Hispanic World* (Oxford, 1982), 66–95, provides a discussion of the events leading to the reinitiation of hostilities after 1621.



FIGURE 1: "Gold above Silver, Honor above All." Among the exploits of the Dutch naval hero Pieter (Piet) Pieters Hein (1577–1629), the taking of Salvador (lower cartouche) ranked second only to his capture of an entire Spanish silver fleet off Matanzas, Cuba, in 1628 (upper cartouche). Irene de Groot and Robert Vorstman, *Sailing Ships: Prints by Dutch Masters from the Sixteenth to the Nineteenth Century* (New York, 1980), plate 57.

Narbona y Zúñiga, also reflected an official version of events. This official "Spanish" version was synthesized and presented by Gonzalo de Céspedes y Meneses in his *Historia de Don Felipe III* (1634). The commander of the Portuguese fleet, Dom Manuel de Meneses, a scholarly soldier with experience in India, also served as royal chronicler of Portugal. His history received official support but was perhaps more frank about the issue of Spanish and Portuguese rivalry than had been expected.<sup>14</sup> Meneses's insider's view of events was amplified by parallel accounts written by Portuguese Jesuit Bartolomeu Guerreiro and by that extraordinary Jesuit, Padre Antônio Vieira, who lived in Bahia during these events and whose moving description of them in the Brazilian Jesuit annual letter of 1627 was the first of his many writings.<sup>15</sup> Together, these accounts present a detailed version of events that historians at the time incorporated into an explanation widely consumed and believed. What is present in all the accounts and what is missing from some provide a key to understanding the preoccupations of those who celebrated the triumph and those who needed to have it celebrated.

IN A PERIOD OF VARIOUS MISERIES, many Portuguese had considered the fall of Bahia a particular calamity because of the loss of its commerce, "which was the greatest and of the greatest profit that this kingdom had," as diarist Pedro Rodrigues (Roiz) Soares remarked. Eyes and hearts turned toward heaven. Soares also reported that the Eucharist was displayed in all the churches, special processions were staged, and prayers were offered. The king ordered the archbishop of Lisbon to have a novena said to Our Lady, and, during the daily masses, the preeminent preachers in the kingdom exhorted the populace to defend the "honor of God and their name as Portuguese, in imitation of their ancestors." When, on November 22, 1625, the Portuguese fleet departed, Soares could report that it carried "all the nobility of this kingdom."<sup>16</sup>

That the Habsburg crown was anxious to retake the capital of the Brazilian sugar colony, a point from which the Dutch might threaten the south Atlantic route to India or even the silver mines of Peru, is certainly understandable. Most remarkable in the mobilization, however, was the response and cooperation of the Portuguese nobility and their enthusiasm to prove their loyalty and willingness to take up arms for the restoration of this colony. The great nobles, the duke of Caminha, the marquis of Villa Real, and the marquis of Castelo Rodrigo, contributed money or armed ships and soldiers at their own expense. The duke of Bragança, the leading Portuguese noble and possible pretender to the throne, was also a major contributor, and his services were later singled out for praise.

<sup>14</sup> The Council of State acted to prevent the publication of other accounts of the expedition before Meneses could publish his. See *Consulta*, Council of State, April 23, 1623, British Library, London (hereafter, BL), Egerton 324, fol. 18.

<sup>15</sup> Vieira's report has appeared in various editions. Compare *A Invasão holandesa da Bahia* (Bahia, 1955); "Annua ou annaes da provincia do Brasil," *Anais da Biblioteca Nacional do Rio de Janeiro*, 19 (1897): 175–218. Vieira later became a major figure during the Portuguese Restoration as an advisor to João IV.

<sup>16</sup> Lopes de Almeida, *Memorial de Pedro Roiz Soares*, 465–67.



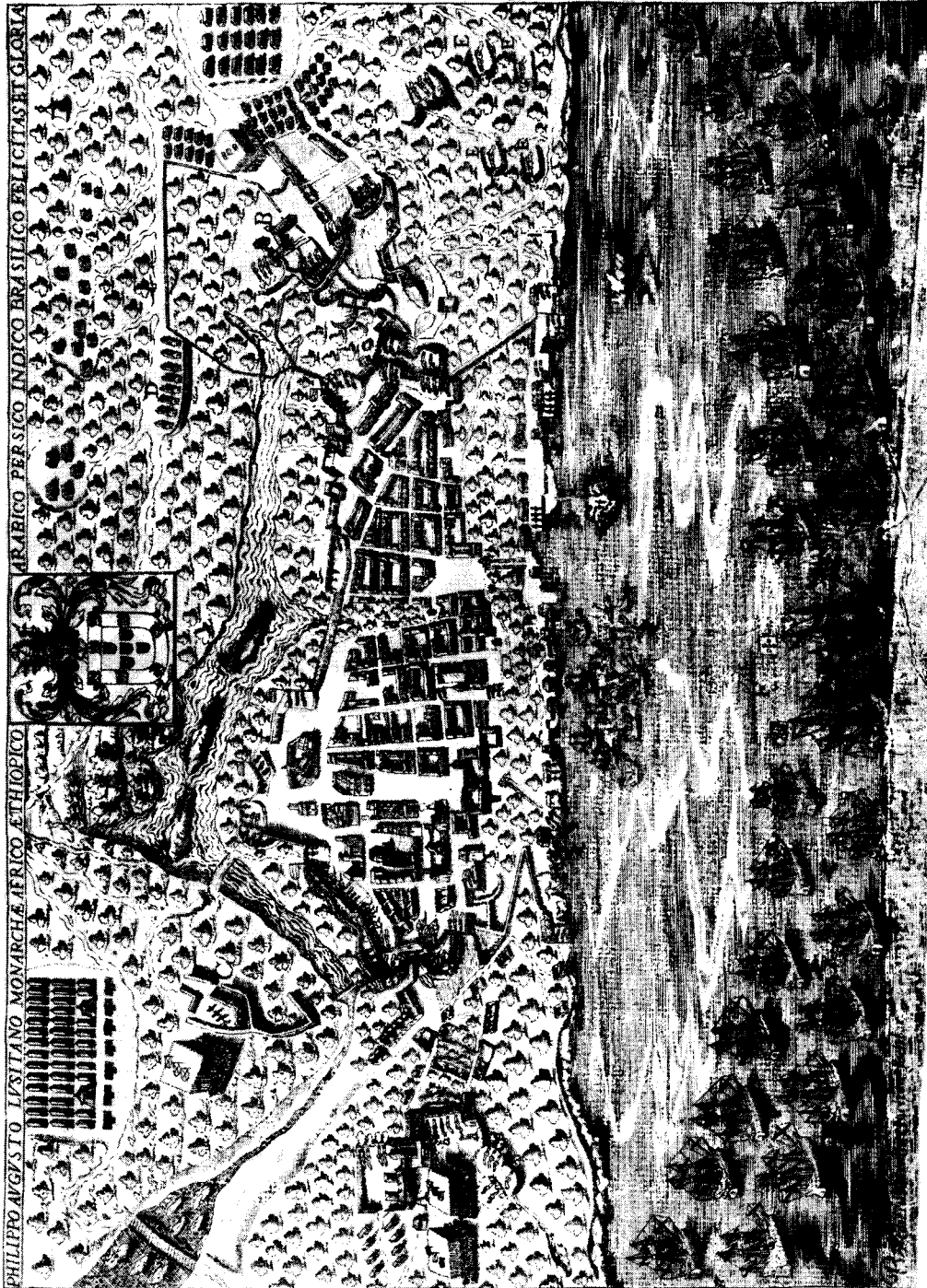


FIGURE 2: The joint armada and the siege of the Dutch at Salvador, Brazil, from Bartolomeu Guerreiro, *Jornada dos Vassallos da Coroa de Portugal* (Lisbon, 1625). This view from the sea bears comparison with that from the land presented in Figure 3.



FIGURE 3: An anonymous canvas, *The Recapture of Bahia*, in a private collection in Seville. Jonathan Brown and John H. Elliott, *A Palace for the King* (New Haven, Conn., 1980), 186. Courtesy Jonathan Brown Institute of Fine Arts, New York University.



Although he did not set sail with the armada, other nobles joined in. Dom Afonso de Noronha, count of São João and member of the Council of State, embarked with his eldest son and heir. The counts of Vimioso, Tarouca, and São João de Pesqueira and the grandson and heir of the count of Villafranca went in person as did Duarte de Albuquerque Coelho, lord proprietor (donatary) of Pernambuco. The sons of former governors of Brazil, Angola, of the viceroy of India, and of the governors of Portugal itself sailed as did the sons and sometimes heirs of high government officials: councillors of state, the royal treasurer (Veedor da Fazenda), and the president of the Supreme Court (Desembargo do Paço). The lords of various places, Bayão, Castello Dalmoural, Pombeiro, Alcaçovas, commanders in the military orders of knighthood, all added luster to the rolls of *fidalgos* who joined the expedition, but, as might be expected, the majority of *fidalgos* who sailed were second sons and lesser nobles whose military risks did not imperil the familial succession of a noble house.<sup>17</sup>

The physical absence of the greatest Portuguese magnates perhaps indicates a certain caution on their part, but contemporary accounts invariably emphasized the adherence of the Portuguese nobility to the project and its broadly popular nature. Manuel de Meneses, commander of the Portuguese fleet and chronicler of the expedition, emphasized the widespread popularity of the venture by recounting anecdotes: a father and son who argued over which one should remain at home because both wished to volunteer; the mother who sent her only son; three noble brothers who enlisted together, only one of whom was to return; the twelve-year-old noble youth who marched with twenty men to enlist and was outspoken in his anger and disappointment when denied passage with the fleet. These stories were the stuff of imagination and perhaps literary license, but almost all the accounts from both the Spanish and Portuguese sides spoke of the distinction and rank of the participants and of their willingness to serve.<sup>18</sup>

The Spanish forces were also distinguished, including "many captains and soldiers from Flanders, Italy, and elsewhere and many gentlemen, all very brilliant," but the quality of the Portuguese forces was most remarkable.<sup>19</sup> Meneses, the Portuguese commander, claimed that Spaniards and Portuguese competed to serve God, the king, and the reputation of the crown in this venture, which he believed was "among the most important, if not the most important of our times."<sup>20</sup> He added that never had so many nobles enlisted in a military venture in which the king himself did not command. Many of the published and

<sup>17</sup> Despite the absence of many titled nobles, the number of *fidalgos* that embarked captured the popular imagination. The many published accounts almost invariably commented on the number of nobles in the armada, and detailed lists were prepared giving the name and rank of each *fidalgo*, his relation to a titled noble or high royal officer, and the ship in which he sailed. See, for example, "Relação dos fidalgos q. vão embarcados na armada q. vai ao socorro ao Brasil," Biblioteca da Ajuda, Lisbon, 51-III-49, fols. 61v-70; "Memória dos fidalgos no anno de 1624 passarão a restauração da Bahia cabeça do Brasil," Biblioteca Nacional de Lisboa (hereafter, BNL), Coleção Pombalina 648, fols. 170-71.

<sup>18</sup> Meneses, *Recuperação*, 381, 383.

<sup>19</sup> Gonzalo Céspedes y Meneses, *Historia de Don Felipe III, Rey de las Españas* (Barcelona, 1634), 205; Valencia y Guzmán, *Compendio historial*, 81.

<sup>20</sup> Meneses, *Recuperação*, 383. The opportunities to act "nobly" are noted in "Lembrança que o Marischal Fernão Coutinho deu por escrito a seus filhos D. Alvaro, e D. Francisco partindo elles para a jornada da Baía" (September 26, 1624), BL, Add. Ms. 20934, fols. 126v-131.

unpublished accounts listed the noble volunteers ship by ship. Surely, there had been nothing like this in Portugal since the impetuous Dom Sebastião led his ill-fated crusade to Morocco in 1577 and scattered the Portuguese nobility on the sands of Alcácer El-Kebir.

Whatever moved the Portuguese nobility to enlist in this venture, it was certainly not the attractions of Brazil. The nobility had never been drawn to the overseas colonies in great numbers. India, Africa and Brazil even more so, represented only sacrifice and danger with little hope of glory or riches. The local societies in these colonies did not represent the full social hierarchies of Portugal, and *fidalgos* objected to these places dominated by missionaries and royal magistrates. The lands of "catechism and lawcodes" (*catecismo e ordenações*) were not for a nobleman seeking recognition and reward. A commission in the Lowlands or on some other European battlefield or a place at court in Madrid was preferable. As positions of high command, the viceroyalty of India and the governorship of Brazil usually remained reserved for important nobles, but service abroad was considered temporary. Nobles who went to the colonies were few and usually transient.<sup>21</sup>

But the expedition to retake Bahia was another matter. The crown had made it clear from the outset that this was to be a major military venture, organized with the full support and interest of the king, and that he was willing to grant many concessions and privileges to nobles who participated. At first, Philip IV had supposedly hoped to command the expedition in person, but his advisers dissuaded him.<sup>22</sup> To replace the honor of serving directly with the monarch, all *fidalgos* who joined the enterprise were promised a general grant (*mercê*) to be enjoyed by their heirs in case of their death on the battlefield. The happy outcome of the expedition led the crown to honor these promises with seeming good will.<sup>23</sup>

It is clear that the Portuguese nobility adopted the Bahian rescue as its enterprise, joining it with enthusiasm and perhaps the hope that, through this venture, they might secure royal favor and recognition of the importance of their nation and their estate. The count of Ericeira, historian of the Portuguese Restoration, wrote that a personal appeal in the king's own hand had come to the governors of Portugal calling on the noted valor and fidelity of the Portuguese in this crisis. "The Portuguese, seeing themselves less despised by the king, showed how well they could act if favored."<sup>24</sup> The noble response for relief of Bahia justly came to be known and celebrated as the "*jornada* (expedition or voyage) of the vassals." In political discourse, the term "vassal" had become synonymous with "subject" but was no longer commonly used to describe the specific relationship of

<sup>21</sup> Eduardo d'Oliveira França, *Portugal na época da restauração* (São Paulo, 1951), 140–42, 172, 190. Virginia Rau, "Fortunas ultramarinas e a nobreza portuguesa no século XVII," *Revista portuguesa de história*, 13 (1959): 1–25, suggests some noble interest and investment in the colonies, but there is no study in depth on this question.

<sup>22</sup> Bartolomeu Guerreiro, *Jornada dos vassallos da coroa de Portugal* (Rio de Janeiro, 1966), 28–29.

<sup>23</sup> Concession to the heirs of Martim Afonso de Oliveira, Arquivo Nacional da Torre do Tombo, Lisbon, Mesa da Consciência, livro de registro 30; Mss. da livraria, liv. 1113, fol. 98; King to Duke of Villahermosa, Madrid, September 17, 1625, BL, Egerton Ms. 1133, fol. 1. Even the count of Ericeira admitted that the "grants were considerable, affecting almost all the principal people who went on the expedition and resulting in great benefit"; *História do Portugal restaurado*, 4 vols. (Lisbon, 1945), 1: 69.

<sup>24</sup> Ericeira, *Portugal restaurado*, 1: 67.

the nobility to the crown. Its use in description of the Bahian venture was not simply an anachronism but a political statement about the relationship between the nobility and the crown.<sup>25</sup>

WHAT MAKES THE PORTUGUESE NOBILITY'S RESPONSE in 1624 noteworthy is the marked contrast to its behavior in the following decade. By the 1630s, the nobles were repeatedly called upon to offer military service to the crown: to recover Pernambuco, to serve in the Atlantic fleets, to campaign in Italy or the Lowlands, and in 1640, to aid in the suppression of the Catalan revolt. Invariably, as a group, the *fidalgos* showed themselves unwilling or reluctant to risk life or fortune in these projects, even those in defense of Portuguese territories and interests. Appeals to neither their national pride, past glories, nor their military responsibilities could overcome their reluctance to serve, an attitude they shared with municipal councils and the rest of the population, which bore the brunt of the financial burden.<sup>26</sup>

Olivares became particularly frustrated while mounting a response to the Dutch seizure of northeastern Brazil after 1630. Shortages of men and ships for the Atlantic fleet and the Indies silver fleets led to difficult choices between Spanish and Portuguese interests.<sup>27</sup> Attempts to create a new armada for Brazil in 1634 were thwarted by Portuguese recalcitrance. In Oporto, sailors who had avoided enlistment and fled were even threatened with seizure of their property and the imprisonment of their wives.<sup>28</sup> In June and July 1634, royal letters went out to the municipal councils of the major cities of Portugal lamenting the crown's many responsibilities and seeking to raise money for an armada to relieve Brazil.<sup>29</sup> These efforts were met with hostility, despite the continual attempts to emphasize that the target of these funds was the recapture of Portugal's own possessions. When in 1640 Olivares sought in one stroke to resolve a political and a military problem by calling upon João, the duke of Bragança, to lead the Portuguese

<sup>25</sup> The term "vassal" came into the Iberian peninsula in the tenth century but did not become common until the twelfth. On the usage of the term in Portugal, see José Mattoso, "O Lexico feudal," in *En torno al feudalismo hispanico: I Congreso de Estudios Medievales* (Madrid, 1982), 295–312. A contemporary Spanish definition of vassal was "whoever is subject to a natural lord by nature of origin or residence." See Juan Robles, *Tardes del Alcázar: Doctrina para perfecto vasallo* (1636; Seville, 1948), 33. The generalized usage of the term can be seen in an appeal of blacks and mulattoes of Piura, Peru, who wished to be excused from paying tribute because they were free and had "only the obligations of loyal vassals"; Archivo General de Indias, Seville, Lima 6, August 28, 1641.

<sup>26</sup> Royal appeals to the Portuguese municipal councils for money were couched in terms of helping recapture Pernambuco and Mina, "the first patrimony of that Crown . . . to retain the profit from the trade of Guinea and the Angolan coast, and to preserve the efforts of the past kings of Portugal and the Portuguese name in its farflung conquests and most of all to maintain the souls won for Christianity." None of these seemed to make the request for 500,000 *crúzados* any more palatable. See King to the *Câmaras* of Portugal, Madrid, September 16, 1633, Arquivo Histórico da Câmara Municipal de Lisboa, Livro 62, fols. 205–06.

<sup>27</sup> Carla Rahn Phillips, *Six Galleons for the King of Spain* (Baltimore, Md., 1986), 196. On Spanish frustration with Portuguese opposition to taxes to pay for imperial defense, see Stuart B. Schwartz, "Luso-Spanish Relations in Hapsburg Brazil, 1580–1640," *The Americas*, 25 (1968): 33–47.

<sup>28</sup> AHU, Bahia papéis avulsos caixa 2, January 20, 1635.

<sup>29</sup> Arquivo Histórico da Câmara Municipal de Lisboa, Livro 62, fols. 210–10v; Arquivo Municipal de Coimbra, King to *Câmara*, Madrid, July 5, 1634.

nobility against Catalonia, the decision proved to be merely a further reason for complaint and a contributing cause for rebellion.

The traditional histories of the Portuguese Restoration have generally ascribed the reasons for such recalcitrance to national sentiments, but, in fact, the causes were at once specifically Portuguese and more generally European in origin.<sup>30</sup> The Portuguese nobility, like aristocracies elsewhere in Europe, were increasingly less willing to take up arms. The ardent justification of noble privilege based on corporate responsibilities as warriors according to the medieval social theories of functional and juridical estates remained intact in the seventeenth century. As Yves-Marie Bercé has stated, "The military calling was common to all Europe's nobility; it was their most visible and enduring characteristic."<sup>31</sup> But the reality of a noble estate penetrated by bourgeois and bureaucratic elements undercut the theoretical basis for their status.<sup>32</sup>

There were other reasons as well. The increasing professionalization of the military and the democratization of death by improved technology made the role of the noble occasional soldier superfluous and unhealthy.<sup>33</sup> In Spain and Portugal, the 1630s witnessed a general reluctance of nobles to fulfill their military obligations.<sup>34</sup> Olivares had found Spanish nobles and members of the military orders increasingly difficult to enlist, and, despite a series of administrative measures and fiscal penalties, the nobility found ways not only to avoid direct military service but even to refuse payments for replacements in the ranks.<sup>35</sup> This problem could be found at all levels from simple hidalgos to the grandees. Symbolically, the commander of the Bahian victory, Don Fadrique de Toledo, himself refused to lead a new expedition to Brazil in 1634, although his reluctance was to some extent motivated by his antipathy toward Olivares and his perception that the forces promised him were unequal to the task.

The collapse of military obligations in Portugal and its empire paralleled the Spanish situation. Increasing royal demands for sectors of the nobility to assume

<sup>30</sup> Luís Reis Torgal, "A Restauração, Breves reflexões sobre a sua historiografia," *Revista da história das idéias*, 1 (1977): 23–40. The classic account of the Habsburg period in Portugal is still Luís Augusto Rebello da Silva, *História de Portugal nos séculos XVII e XVIII*, 5 vols. (Lisbon, 1860–71). An excellent modern study is Antônio de Oliveira, *Poder e oposição política em Portugal no período filipino (1580–1640)* (Lisbon, 1990).

<sup>31</sup> Yves-Marie Bercé, *Revolt and Revolution in Early Modern Europe*, Joseph Bergin, trans. (New York, 1987).

<sup>32</sup> David Bitton, *The French Nobility in Crisis, 1560–1640* (Stanford, Calif., 1969), 40–41. Portugal had also created a bureaucratic class that increasingly sought noble status. I have discussed this in Stuart B. Schwartz, *Sovereignty and Society in Colonial Brazil* (Berkeley, Calif., 1973), 68–94, 280–313; see also Perez Zagorin, *Rebels and Rulers*, 1: 76–77.

<sup>33</sup> Compare Lawrence Stone, *The Crisis of the Aristocracy, 1558–1641* (Oxford, 1967), 129–31.

<sup>34</sup> This is a trend noted elsewhere in Europe. The Castilian nobility had demonstrated a reluctance to undertake military obligations in 1635 and again in 1640. See John Elliott, *The Count-Duke of Olivares* (New Haven, Conn., 1986), 509–10; Antonio Domínguez Ortiz, "La Movilización de la nobleza castellana en 1640," *Anuario de historia del derecho español*, 25 (1955): 799–824. The process is also traced in L. P. Wright, "The Military Orders in Sixteenth and Seventeenth Century Spanish Society," *Past and Present*, 43 (1969): 55–65.

<sup>35</sup> Domínguez Ortiz, "La Movilización," provided considerable detail on this problem. He accused Olivares of being particularly unrealistic in his expectation of military service from large numbers of hidalgos who had long before abandoned any pretense to military virtues or responsibilities. See also Elliott, *Count-Duke of Olivares*, 509.

their military obligations were met by heel-dragging, a clear indication of the changes in the relation of royal authority and noble obligation.

The Portuguese military orders provide a case in point. The Habsburg rulers of Portugal had been relatively liberal in distributing places (*habits*) in the military-religious orders. Over 1,800 new knighthoods had been created during the reigns of Philip II and Philip III, but there had been tensions between Spanish and Portuguese knights over the reciprocity of their privileges.<sup>36</sup> By 1635, the Dutch occupation of northeastern Brazil and the entrance of France into the war against Spain had created a desperate need for recruits and funds. The king's inability to muster a sufficient force for a fleet to retake Brazil moved the crown to form a special junta of the Council of Portugal to examine the legal basis by which the king could require members of the military orders to serve in Brazil.<sup>37</sup> The junta argued that the king as perpetual governor and administrator of the orders could require knights to serve in person or to supply a replacement in the armada because of their obligation to fight against the infidel. Moreover, knights holding income-earning benefices who did not serve in person were required to return a portion of their annual stipend (one-fourth for the Order of Christ, one-sixth for Aviz and Santiago) to pay for the war.

The military orders resisted royal pressure through the Board of Conscience (*Mesa da Consciência*), a royal council responsible for ecclesiastic and moral issues. Jealous of its powers, the board claimed that only it had the authority to compel compliance of the military orders and that the king's power over the orders as granted through various papal bulls was limited. The junta looked on this reluctance as unworthy of knights pledged to resist the infidel, especially in view of the expenses incurred by the crown and the tax burden carried by the population as a whole.<sup>38</sup> Unlike the later mobilization for Catalonia, which was resisted because it required action beyond the boundaries of Portugal and its empire, refusal to serve in Brazil spoke to other concerns.

Despite the role of the king as Grand Master of the Military Orders of Portugal, which gave the dispute the tenor of an internal problem, the conflict was essentially over the feudal obligation of the knights to offer *auxilium et concilium* according to the ancient formula.<sup>39</sup> Here was a classic institution of the late Middle Ages, the religious-military order, that no longer performed its stated function. The crown's own policy of awarding *habits* in the orders to those with bureaucratic, courtly, or mercantile careers contributed to this situation, a

<sup>36</sup> See Francis A. Dutra, "Membership in the Order of Christ in the Sixteenth Century: Problems and Perspectives" (unpublished typescript); also his "Membership in the Order of Christ in the Seventeenth Century: Its Rights, Privileges, and Obligations," *The Americas*, 27 (July 1970): 3–25. Wright, "Military Orders," presented evidence from the Order of Santiago in Spain of the inflation of honors under Olivares. Between 1621 and 1640, 1,746 *habits* in that order were awarded, three times the number granted in the previous twenty years.

<sup>37</sup> I have presented a more detailed analysis of this material in "Colonial Brazil: The Role of the State in a Slave Social Formation," in Karen Spalding, ed., *Essays in the Political, Economic and Social History of Colonial Latin America* (Newark, Del., 1982), 1–24.

<sup>38</sup> *Consulta*, Consejo de Portugal, AGS, Secretarias provinciales 1583, fols. 583–86, November 1636. Biblioteca Nacional, Madrid (hereafter, BNM), Papeles de Orden de Christo 938, fols. 202–10; *Consulta*, Mesa da Consciência, BNL, Fundo geral 7636, fols. 1–6.

<sup>39</sup> F. L. Ganshoff, *Feudalism* (New York, 1952), 135–40. See also the sources cited in Schwartz, "Colonial Brazil," 21, nn. 27–32.



circumstance that had not gone unnoticed, as a longstanding debate over “arms versus letters” underscored.<sup>40</sup> In a sense, the crown now had to live with the kind of nobility it had created. The celebration of the loyalty and military contribution of the nobility at Bahia was a means to exhort and to convince by example at exactly the time when such cooperation was becoming increasingly difficult to obtain.<sup>41</sup>

While the nature of the “feudal” military obligations of the knights to the crown was obscured to some extent by the position of the king as Grand Master, no such mediating relationship stood between the crown and the donataries, or lord proprietors, of the Brazilian colony. The same needs that impelled the crown to mobilize the knights of the military orders moved it to force these nobles to play a more active role in defense of their grants. Beginning in 1618 and especially after 1625, as it became clear that the Dutch West India Company had designs on Brazil, the crown sought to force the donataries to use their “persons and their wealth” to protect their captaincies. The count of Monsanto, the marquis of Porto Seguro, and Duarte Coelho de Albuquerque, proprietor of Pernambuco, were all asked to send aid to Brazil.

By 1634, the Council of Portugal was willing to take other measures. It authorized a full judicial review of the original charters in order to find the basis on which the assumed military obligations of the proprietors could be enforced. Tomé Pinheiro da Veiga, the royal procurator, and Jorge de Cabedo, a leading jurisconsult, were set to the task of making the crown’s case. They found that the original titles were all of *juro e herdade* (compensation and inheritance) and did not bear obligations of an act of fealty or military service. They were simple grants in reward for past service. But, realizing his sovereign’s needs, da Veiga argued that since the donataries also held the title of captain, they had obligations similar to those of castellans (*alcaldes-mores*) to prepare effective defenses.<sup>42</sup> This bit of legal legerdemain designed to answer the crown’s needs demonstrated the limitation of even seemingly “feudal” grants. It was not the donataries’ roles as territorial lords but as officeholders that the lawyers sought to use to justify their service. The “feudal” argument was becoming an anachronism.

In effect, the “voyage of the vassals” was the last great enterprise in the Iberian world in which the traditional feudal obligations and military values of the nobility were effectively mobilized by the crown. In Portugal, while some rewards were forthcoming, the continuing absence of the court, a worsening colonial situation, and dissatisfaction with Spanish policies contributed to a weakening of the relation between the crown and the nobility as a class, or at least some portion of it. The expedition marked a break in the transition of the seventeenth century, a historical moment in the transformation of European society and political organization.

<sup>40</sup> The debate enters *Don Quixote*, but more important in this context is João Pinto Ribeiro, *Preferência das armas às letras* (Lisbon, 1645). Luys Coello de Barbuda, *Empresas militares de Lusitanos* (Lisbon, 1624), written in Spanish, is an attempt to demonstrate the military virtues of the Portuguese.

<sup>41</sup> Céspedes y Meneses, *Historia de Don Felipe III*, which follows the official version closely, was published in 1634 in the midst of the mobilization difficulties.

<sup>42</sup> BNL, Fundo geral 7626, fols. 41–43.



AS THE BAHIAN EXPEDITION provided one set of lessons to the nobility, it offered quite a different one to Olivares. The victories of 1625 at Breda and Cadiz produced an atmosphere of ebullience in Madrid. The victory at Salvador, with its successful cooperation between Castilians, Portuguese, and Neopolitans, was especially prized. Plans for the "Union of Arms" decreed in July 1626 took the Bahian expedition as a primary example of the advantages of an integrated military force.<sup>43</sup> The desire of Olivares to rationalize defense and unify law was in keeping with the long-term goals of centralized government, for which he was willing to pay with rewards and offices. But Portuguese resistance to this plan and its proposed quota of 16,000 men for an integrated force was firm. The Portuguese later complained that the "Union of Arms" and the call to fight in "foreign lands" was among the most irksome of Castilian measures.<sup>44</sup> Olivares perhaps believed too literally the message of the "*jornada* of the vassals." Indeed, the various authors probably had emphasized this particular message, knowing it would be well received at court. During the early 1630s, when it became increasingly difficult to mobilize the various parts of the empire for joint efforts, the Bahian venture became a metaphor of hope in a political text of growing frustration. Maino's extraordinary painting of the recapture of Salvador was commissioned in 1633, and Céspedes's recounting in 1634 of the events took great pains to point up the multinational composition of the victorious forces—Aragonese, Andalucians, Portuguese, and Italians—who had all fought bravely and for a common goal.<sup>45</sup> If Olivares had read more skeptically the triumphalist accounts of the *jornada*, especially those of the Portuguese authors, and the official documentation, he might have seen that beneath the outwardly friendly collaboration lay a serious undercurrent of rivalry and competition.

There had been trouble from the outset concerning the integration of the Portuguese and Spanish fleets under the unified command of Don Fadrique and minor difficulties over the precedence of the battle standards of the two crowns. To the chagrin of the Portuguese commander, Meneses, Don Fadrique was also given jurisdiction over the local inhabitants, despite the fact that Bahia was a Portuguese colony. Matters worsened after the Dutch surrendered. The Portuguese troops were left outside the city walls, and the lions and castles of the Castilian flag were raised over the cathedral. Whether this was a matter of inadvertence or of "the hatred of the Castilians for the Portuguese which they demonstrate in everything, but never so publicly," as Meneses wrote, was unclear. In any case, the Portuguese were offended. They composed the majority of the troops, and, unlike the usual conscripts, many of them were nobles and unpaid

<sup>43</sup> Elliott and de la Peña, *Memoriales y cartas*, 1: 173–200. See also Elliott, *Count-Duke of Olivares*, 244–77, which notes the impact of the string of victories from Breda to Cadiz on Olivares's thinking in late 1625; and John Lynch, *Spain under the Hapsburgs*, 2 vols. (New York, 1969), 2: 94–101.

<sup>44</sup> Fernando Jesús Bouza Álvarez, "Teoría y opinión de la Restauração Portuguesa: Del Portugal rebelde al Portugal restaurado" (Licenciate thesis, Universidad Complutense de Madrid, 1982), 133–34. Compare José Antonio Armillas Vicente, "Acción militar del estado aragonés contra Portugal (1475–77 y 1664–65)," *Estudios* (Zaragoza) (1979): 209–29. The most thorough study of the effects of the Union of Arms is Enrique Solano Camón, *Poder monárquico y estado pactista (1625–52)* (Zaragoza, 1987).

<sup>45</sup> Céspedes y Meneses, *Historia de Don Felipe III*, 239.

volunteers.<sup>46</sup> But the worst was yet to come. The Castilians billeted themselves in the remaining houses and proceeded to sack the city “with excessive cruelty” as if it were an enemy town, “respecting neither door nor lock.”<sup>47</sup> Even Spanish observers noted that the city suffered “worse damage from our troops than from the enemy because nothing remained that was not robbed or destroyed despite orders given or guards posted.”<sup>48</sup> Later, there followed disputes over a moratorium on taxes in Brazil, prosecution of collaborators, and the payment of duties on Brazilian sugar, dyewood, and booty taken back to Spain.<sup>49</sup> A garrison of 1,000 troops remained quartered in the city when the fleets returned to Europe, and Don Fadrique held that all local costs incurred were the responsibility of the Portuguese treasury.<sup>50</sup> Don Fadrique’s insensitivity and partisan actions rankled the Portuguese and were not quickly forgotten.<sup>51</sup> The Bahian venture should have demonstrated to Olivares that military integration, no matter what its benefits, could not ignore national differences.

IF THE NOBILITY WERE THE HEROES of the Bahian victory, the Jews were its villains. The mythology of the fall and recapture of Salvador quickly established two “truths” fundamental to an explanation of these events in which social groups took expected and symbolic roles; the loyalty and valor of the nobility and the treason of the “people of the Hebrew nation” (*gente da nação hebréia*). The supposed natural opposition of a nobility, with its inherent claim to honor, and the New Christians who, by definition, had none became the dominant trope in the standard accounts of the events, which presented a history written with specific political and religious objectives.<sup>52</sup>

Curiously linked to the role of the New Christians was the performance of Dom Marcos Teixeira, bishop of Bahia and formerly an inquisitor in Portugal. A powerful and headstrong man, Teixeira had begun to feud incessantly with local

<sup>46</sup> Meneses, *Recuperação*, 591; Guerreiro, *Jornada*, 111.

<sup>47</sup> Meneses, *Recuperação*, 592; Letter of Manuel do Rego Siqueira, procurator of the *Câmara* of Salvador, AHU, Bahia papéis avulsos caixa 1 (nos. 2–5). The local residents sought a tax exemption as a result of these conditions. See also Boxer, *Salvador de Sá*, 62.

<sup>48</sup> “Relación breve del sucesso del Brasil contra los holandeses,” BNM, Ms. 2356, fols. 414–15. Céspedes y Meneses, *Historia de Don Felipe III*, 242, argued that looting took place despite attempts to stop it and that, in any case, “military license is rarely restrained in that which it judges its reward.”

<sup>49</sup> A Portuguese ship captain, Gonçalo Maciel da Guarda, lost his vessel, cargo, and guns when the Dutch took the city, and, after fighting for a year in the guerrilla campaign and aiding in the recapture of the city, his request for return of the lost possessions was denied by Don Fadrique, who claimed they were taken in “buena guerra”; AGS, Secretarias provinciales 1468, fols. 289–93. The governors of Portugal wanted returning ships carrying Brazilian products to call at Lisbon to pay duties, but influential members of the Council of State such as the count of Puebla and the duke of Villahermosa claimed that these were spoils of war. Compare *Consulta*, Council of State, October 23, 1625, BL, Egerton 1131, fols. 322–23; and *Consulta*, Junta de Guerra, BL, Egerton 1131, fol. 321.

<sup>50</sup> Meneses, *Recuperação*, 551–52.

<sup>51</sup> Ericeira, *Portugal restaurado*, 1: 69, noted these events bitterly.

<sup>52</sup> On the complicity of the New Christians in the Bahian affair, see Anita Novinsky’s excellent *Cristãos novos na Bahia* (São Paulo, 1972). The most thorough examination of the problem is Eduardo d’Oliveira França, “Um Problema: A Traição dos cristãos novos em 1624,” *Revista de história*, 41: 83 (1970): 21–71, which demonstrates the lack of evidence of complicity but suggests why it should have occurred. See also Anita Novinsky, “A Historical Bias: The New Christian Collaboration with the Dutch Invaders of Brazil,” *Fifth World Congress of Jewish Studies* (1972), 2: 141–54.

civil authorities from the moment he set foot in Brazil.<sup>53</sup> When news of the impending Dutch attack was received, Teixeira added his voice to those of the planters who thought mobilization was alarmist. Invited to bless the cornerstone of a newly constructed fort in 1624, he said he would rather damn it, since it had diverted funds from the building of his cathedral.<sup>54</sup> Such attitudes made his subsequent actions particularly ironic.

Upon the arrival of the Dutch, the bishop buckled on breastplate and arms and strode about the city confessing soldiers and urging defense to the death. After the initial assault, on the night of May 9, panic took hold of the defenders, and the bishop sought to leave the city. Despite the efforts of the rector of the Jesuit College to convince him otherwise, Teixeira left. When his flight became known, the defense melted away. Some 3,000 troops joined the bishop, and a stream of refugees fled headlong into the interior, leaving the governor, his son, and a few staunch companions to face the surprised invaders the following morning.<sup>55</sup>

Such behavior by a leading churchman and former inquisitor was an embarrassment, and the subsequent histories of these events emphasized the bishop's later role in organizing the resistance of the countryside and kept silent about his actions in the city's surrender.<sup>56</sup> Blame had to be placed elsewhere. Tamayo de Vargas noted that the New Christians "rarely needed much incentive to practice infidelities," and he suggested that their rumor mongering had undercut the defense.<sup>57</sup> But eyewitness accounts, especially those of the Jesuits, with whom Teixeira had good relations, leave no doubt about his flight. Later, in 1626, when Teixeira's brother, Jesuit Father Damião Botelho, sought some reward in recognition of his brother's services, the archbishop of Lisbon as member of the Council of Portugal voted to support a resolution stating that although Teixeira's service in resisting the Dutch was great, it did not compensate for the "disservice he did in the loss of and the manner in which he abandoned the same city."<sup>58</sup> This internal criticism was smothered under a blanket of more public praise for the bishop's actions and simultaneous condemnation of New Christian treason.

While the histories and accounts transformed Teixeira into a heroic soldier-

<sup>53</sup> Teixeira and Governor Diogo de Mendonça Furtado fell out over precedence in the welcoming procession on the day of Teixeira's arrival. Later, there were disputes with royal judges over seating arrangements in the cathedral, over the conflict between civil and ecclesiastical jurisdiction, over payment of the tithe, and over the bishop's obligation to pay a tax on the slaves he imported. See Schwartz, *Sovereignty*, 208–09.

<sup>54</sup> Schwartz, *Sovereignty*, 208–09.

<sup>55</sup> While the contemporaneous accounts differ on details, there is general agreement about the panic and desertion of the city. The Jesuit sources that note the bishop's failure to heed the Jesuits' advice are found in Serafim Leite, *História da Companhia de Jesus no Brasil*, 10 vols. (Lisbon, 1938–50), 5: 28–29. See also Antônio Vieira's "Carta anua," in *Anais da Biblioteca Nacional de Rio de Janeiro*, 19 (1897): 175–217. Vieira's letter is also available as *A Invasão holandesa da Bahia* (Bahia, 1955).

<sup>56</sup> Bishop Teixeira, in fact, deposed Antônio de Mesquita Oliveira, the senior royal magistrate who had taken over when the governor was captured and who organized the resistance in the countryside. This incident also received little discussion in the official histories, although Meneses noted that Mesquita later claimed Teixeira held an old grudge and was by nature ambitious; see Meneses, *Recuperação*, 399.

<sup>57</sup> Tamayo y Vargas, *A Restauração*, 69–70. Céspedes y Meneses, *Historia de Don Felipe III*, picks up this same explanation.

<sup>58</sup> Petition of Father Damião Botelho (1626), AGS, Secretarias provinciales 1468, 617–20.

cleric, the same accounts, especially those by Spanish authors, laid blame for the original defeat squarely on the New Christians. The Dutch may have wanted Brazil because they hoped that old commercial ties with the resident New Christians might prove advantageous, but Dutch reports of the story do not mention New Christian help and make it clear that the Dutch fleet was guided by its own pilots, veterans on the Brazilian coast. Later, the Dutch clergyman Enoch Startenius categorically denied that his fellow countrymen received help from the New Christians.<sup>59</sup> The published eyewitness reports by Vieira and the other Portuguese Jesuits fail to include the New Christians in the events leading to the disaster. Even Meneses's official history, while noting the untrustworthy nature of such people, assigns no major role to them in its explanation of events.<sup>60</sup> But, on the Spanish side, the charge of treason was made immediately with the first reports of Dutch plans. It figured prominently in all the major histories and became the leitmotif of Lope de Vega's *Restoration of Brazil* (*El Brasil restituido*), a play written for performance at court and based on the first fragmentary news of the victory.

The first suggestion of complicity came from a Flemish resident in Oporto. Henrique Sinel claimed that his Amsterdam contacts had informed him of the treason. His self-interested charges received support from a confession extracted by torture from a New Christian in Pernambuco. When word of the city's loss reached Spain in July, a special junta with representatives from the Councils of State, War, and Portugal met in Madrid in August 1624 to plan a response.<sup>61</sup> While some, like Mendo da Mota, argued that there was no hope so long as there were internal enemies and that all New Christians should be expelled from the colony, his colleague Francisco Pereira held that "there is little evidence Bahia was taken by the treason of the New Christians."<sup>62</sup> The lack of evidence could not dispel a deep-rooted tradition of distrust and expectation of treason.<sup>63</sup> Political treachery motivated by greed or hatred of Christianity was a standard accusation against the New Christians. It had been made long before Bahia's fall and continued to be made afterward.<sup>64</sup>

<sup>59</sup> See Boxer, *Salvador de Sá*, 51. Startenius was no defender of the New Christians and claimed that the Dutch despised them as much as did the Spanish Catholics. He implied that the Portuguese attitude was more lenient. See *Colección de documentos inéditos para la historia de España*, 4: 177–78.

<sup>60</sup> Oliveira França, "A Traição," 24–27, pointed out the distinction between the Spanish and Portuguese accounts. In fact, some of the lesser Portuguese accounts do make virulent anti-New Christian charges, but these seem based more on hearsay than observation. See, for example, "Relação verdadeira," *Revista do Instituto Histórico e Geográfico Brasileiro*, 20 (1844): 476–89.

<sup>61</sup> *Consulta*, August 2, 1624, BL, Egerton 1131, fols. 290–304. Sinel's information led to another meeting on the New Christian problem on September 2, 1624; BL, Egerton 1131, fol. 272. Sinel was not a disinterested party and hoped to profit. He offered to build twelve galleons in Viscaya "on contract" to meet the Dutch threat. *Consulta*, Council of War, AGS, Guerra antigua, 901.

<sup>62</sup> "tiene por pouca justificada de ser entrada la Bahia por traición de los cristianos nuevos"; BL, Egerton 1131, fol. 272. Mota was following the information provided by Sinel.

<sup>63</sup> There were certainly good reasons for the New Christians to collaborate with the Dutch. Bahia was a Dutch target because it had "many New Christians of the Hebrew nation desirous to seek any change that would allow them to practice the law of Moses"; see Juan de Valencia y Guzmán, *Compendio historial de la jornada del Brasil*, in *Colección de documentos inéditos para la historia de España*, 55 (1870): 63.

<sup>64</sup> For example, in a series of suggestions made ca. 1580, a Portuguese memorialist argued for the expulsion of the New Christians from all the colonies: "this nation is very prejudicial in all the conquests because of the bad example they set and the New Christians were stimulating rebellions as

The loyalty of the nobles, heroism of the bishop, and perfidy of the New Christians provided a series of moral examples to explain the events. The fact that Bishop Teixeira died during the year of guerrilla fighting not only made him a martyr but also led to claims that a New Christian physician had poisoned him, "because due to our sins, only they practice medicine."<sup>65</sup> This provided an opportunity to link factually the church's defense of the realm with the Jews' betrayal of it. Other unsubstantiated rumors also circulated widely.<sup>66</sup> When justice was meted out after the city was retaken, five New Christians and some black slaves who had cooperated with the Dutch during the occupation were executed, and a number of Portuguese were punished for collaboration or contact with the enemy. In fact, a later inquest charged twenty-two persons with contacting the Dutch invaders, but only six were New Christians.<sup>67</sup> Still, the "stab in the back" theory circulated widely and helped to exculpate the defenders for the loss of the city in the first place.

THE SOCIAL AND POLITICAL context of these events explains a great deal about the treatment of the central themes in their reporting. The "voyage of the vassals" took place in the heat of a long social war between the New Christians and those who feared and hated them for religious, economic, and political reasons.<sup>68</sup> At various points, this conflict pitted the Inquisition and the nobility, as the major defenders of orthodoxy and "purity of blood," against the New Christians, whose strength lay in their mercantile skill and the financial service they could provide a crown in constant need of money. The question was thus raised in a practical way of the relative weight of religious ideology and traditional interest on the one hand and *raison d'état* and an emerging mercantilism on the other.<sup>69</sup> Of course, not all New Christians were merchants, but a large majority of merchants were New Christians. A study of 364 merchants who operated in Lisbon in the

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they had done in Flanders"; AGS, Estado 416. In 1630, the marquis of Oropesa wrote that Pernambuco had been lost "por mediado hebreo"; *Discurso*, BNM, Ms. 18719, n. 41.

<sup>65</sup> Fray Francisco de São Bras, who claimed to be an eyewitness to events, stated that the bishop had a Jewish doctor who purged, bled, and finally poisoned him. The poisoning charge appears elsewhere, but Father Vieira simply reported that the bishop fell ill and died. São Bras appears also to be the author of a particularly anti-Semitic description of events filled with other uncorroborated charges. Compare "Relação da perda da Bahia," BNM, Ms. 17533; Real Academia de Historia de Madrid, 9-5-1/656, Colección Salazar K-61, fols. 248-49v. On the widespread belief that New Christian doctors murdered their Old Christian patients, see the sources discussed in João Lucio de Azevedo, *História dos cristãos novos portugueses*, 2d edn. (Lisbon, 1975), 167-68.

<sup>66</sup> *Relação verdadeira de todo o succedido na restauração da Bahia . . .* (Lisbon, 1625), in *Revista do Instituto Histórico e Geográfico Brasileiro*, 20 (1844): 476-90.

<sup>67</sup> Novinsky, "Historical Bias," 146, referring to the inquest of Manoel Themudo, Arquivo Nacional da Torre do Tombo, Caderno do Promotor de Lisboa, n. 10.

<sup>68</sup> José Veiga Torres, "Uma longa guerra social: Os Rítmos da repressão inquisitorial em Portugal," *Revista de história econômica e social*, 1 (1978): 55-68. Joaquim Romero Magalhães, "Em busca dos 'tempos' da inquisição (1573-1615)," *Revista da história das idéias*, 9 (1987): 191-228, argued that the period to 1605 was one of reorganization within the Inquisition, resulting in a new strength for the period of its great activity after 1620. During this period, cases of Judaizing made up between 84 and 90 percent of the Portuguese tribunals' activities.

<sup>69</sup> José A. Fernández-Santamaria, "Reason of State and Statecraft in Spain (1595-1640)," *Journal of the History of Ideas*, 41 (1980): 355-79.



seventeenth century indicates that New Christians made up 78 percent of their number. Of the roughly 200 merchants operating in Lisbon at any one time, perhaps two-thirds were of this group. In Bahia, roughly half the merchants were New Christians.<sup>70</sup>

The diaspora of Jews and, later, New Christians from Spain and Portugal had by the seventeenth century created a broad network among them that transcended national and religious boundaries. Kinship, established routes, personal contacts, and trust, the bases of early modern trade, provided the "Portuguese" merchants great comparative advantages in an age when instruments of credit and commerce were still insecure. It also made them extremely attractive to governments in need of revenue and increased trade, especially after the initiation of hostilities in 1618. But, whereas some governments were willing to temper their religious animosity toward the Jews in return for potential economic benefit to the state, in Portugal and Spain such an approach met fervent resistance from various sectors of society.

The events just prior to the Dutch attack on Bahia had generated much tension. New Christians had succeeded in their efforts to secure a general pardon, freedom of movement, and some relief from arbitrary arrest and confiscation after the payment of a large sum to the crown in 1605. During the period of the Twelve Years' Truce (1609–1621) between Holland and Spain, evidence that fugitive New Christian and Jewish capital had strengthened the Dutch economy created a climate in Portugal and Spain that encouraged a number of financial advisers to advocate even wider liberties in order to promote commerce and economic growth. Men such as González de Cellorigo and Duarte Gómez Solis put forward aggressive proposals to increase commerce and favor mercantile interests by reducing the restrictions on the New Christians.<sup>71</sup> Such arguments provoked an intense reaction from the Inquisition and the nobility. At the Portuguese Cortes of 1619, the nobility argued that "by illegal means they [New Christians] have grown to such an extent in wealth that they control all action and commerce and by this means are in communication with various nations, enemies of the Faith." The nobility asked that New Christians be prohibited from all positions in the royal bureaucracy and the church, forbidden to study medicine or even Latin, and that they be expelled from the kingdom and the overseas territories.<sup>72</sup>

<sup>70</sup> David Grant Smith, "The Mercantile Class of Portugal and Brazil in the Seventeenth Century: A Socio-Economic Study of the Merchants of Lisbon and Bahia, 1620–1690" (Ph.D. dissertation, University of Texas, 1975), 14–20. Smith's study is the best to date on this problem.

<sup>71</sup> I. S. Révah, "Le Plaidoyer en faveur des 'Nouveaux-Chrétiens' portugais du licencié Martín González de Cellorigo (Madrid, 1619)," *Revue des études juives*, 4th ser., 2 (1963): 279–398. On Gómez Solis, see José Calvet de Magalhães, *História do pensamento econômico em Portugal* (Coimbra, 1967), 196–236; and Carl Hanson, *Economy and Society in Baroque Portugal* (Minneapolis, Minn., 1981), 113–18.

<sup>72</sup> Proposal of the Three Estates to Philip III, BNL, Coleção Pombalina 249, articles 37, 38, 41, 43. The intense debate of the 1620s is reviewed in Salo W. Baron, *A Social and Religious History of the Jews*, 18 vols. (New York, 1937– ), 15: 160–242. The pulpit became an important scene where the position of the Inquisition and of various clerics was expressed on the issue of the New Christians. The published sermons of Antônio de Sousa (Lisbon, 1624) and João de Ceita (Evora, 1624) ascribed most of Portugal's ills to the New Christians. See the discussion and sources cited in João Francisco Marques, *A Parenética portuguesa e a dominação filipina* (Oporto, 1986), 153–55. Antonio Domínguez Ortiz, "Los Judeoconversos en la vida española del Renacimiento," *Estudios de historia económica y social de España* (Granada, 1987), 163–79, argued that the nobility protected the converts in the sixteenth

The resumption of hostilities with Holland, a contraction in the Atlantic economy, and the accession of Philip IV made the question of the New Christians more urgent.<sup>73</sup> The Inquisition defended its actions and argued that it “conserved the kingdom in unity and in royal fidelity” against a Judaism responsible for “all the ills we suffer.”<sup>74</sup> Coordinated autos-da-fé in Oporto, Coimbra, and Lisbon in November 1622 killing 376 prisoners demonstrated the Inquisition’s continuing power. In 1623, Olivares’s *junta de reformación* suggested changes in the “purity of blood” statutes that had established racial criteria for employment and reward. This reform provoked defenders and critics of the statutes to new activity.<sup>75</sup> New Christian representatives traveled to Madrid with a promise to pay 150,000 ducats for an end to secret denunciations, arbitrary confiscation of property, and prohibitions on freedom of travel. They were able to obtain a royal Pragmatic Sanction to this end in 1623.<sup>76</sup> By 1623, Portuguese churchmen were discussing the possibility of total expulsion or creating segregated communities under constant guard, for which the New Christians themselves would have to pay. Inquisitor-General Fernão Martins Mascarenhas argued that the New Christians were “powerful because of their wealth and social position” and that, since they had invested in the Dutch West India Company, the possibility that they conspired with the enemy was real.<sup>77</sup> These discussions were accompanied by denunciations of New Christian wealth and sins from the pulpit and the publication of virulent anti-Semitic tracts.<sup>78</sup>

The seeming New Christian victory in 1623, the bitter reaction of the Inquisition to it, and the possibility of a new royal policy more favorable to the New Christians set the stage for the history of the reconquest of Bahia. A history emphasizing the triumph of Catholic arms and Philip IV as defender of the faith demonstrated the crown’s ability to perform its traditional roles. Even as the crown left unsatisfied the Portuguese nobility’s desire to have the court reside in Lisbon and the Portuguese nobility continued to complain about appropriate rewards, employment, and recognition, the recapture of Bahia gave the crown an opportunity to express its gratitude.<sup>79</sup> Celebration of the loyalty of the Portuguese

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century in the face of popular pressure. By the seventeenth century, this was certainly not the general situation.

<sup>73</sup> Ruggerio Romano, “Between the Sixteenth and Seventeenth Centuries: The Economic Crisis of 1619–22,” in Parker and Smith, eds., *General Crisis*, 165–225; Barry E. Supple, *Commercial Crisis and Change in England, 1600–1642* (Cambridge, 1959), 1–22.

<sup>74</sup> “Defense of the Inquisition,” Hispanic Society of America, Ms. 2/13, fol. 1v.

<sup>75</sup> Henry Kamen, *The Spanish Inquisition* (London, 1965), 133–35. See also Albert Sicoff, *Les Controverses des status de ‘purité de sang’ en Espagne du XV<sup>e</sup> au XVII<sup>e</sup> siècle* (Paris, 1960). The statutes of “purity of blood” which had no basis in Spanish civil law, were under serious attack in this period even within the Inquisition itself.

<sup>76</sup> Antonio Domínguez Ortiz, *Los Conversos de origen judío después de la expulsión* (published in *Estudios de historia social de España*, vol. 3, 108–10).

<sup>77</sup> Azevedo, *História dos cristãos novos*, 184–86. Portuguese Jews in Holland actually contributed less than .05 percent of the capital for the Dutch West India Company. See Israel, *Dutch Republic and the Hispanic World*, 127.

<sup>78</sup> Vicente da Costa Matos, *Breve discurso contra a herética perfídia do judaísmo* (Lisbon, 1623). See the discussion in Azevedo, *História dos cristãos novos*, 178–80; Maria Idalina Resina Rodrigues, “Literatura e anti-semitismo, séculos XVI e XVII,” *Brotéria*, 109 (1979): 41–56, 137–53.

<sup>79</sup> Mendo da Mota to Baltesar de Zúñiga (*sine die* but about 1622) in BL, Egerton 1131, fols. 268–70. Olivares in the *Gran memorial* noted the complaints about the absence of the court and

nobility and reward for their services satisfied the crown's need to appear grateful to its traditional supporters. Moreover, this example had definite advantages for Olivares's program to integrate the monarchy's various parts, since the benefits of collaboration were so clear. How much of Lope de Vega's theatrical rendition of the story was a response to current political opinion at court is uncertain, but his play *El Brasil restituído*, like the chronicles and accounts, placed great emphasis on the valor of the Portuguese and the joint nature of the venture. In the play, on sighting Brazil, Don Fadrique's

aunque nos recibas mal [although you receive us ill,  
Brasil, espero en tu orilla [Brasil, I await upon your shore]  
nombre y laurel inmortal [immortal name and laurel]  
Vivan Felipe y Castilla! [Long live Philip and Castile!]

is answered by Meneses's,

Tierra, tierra, Portugal. [Land ho, Land ho, Portugal.]

In reality, Meneses's own account was never published, despite his position as royal chronicler. It was perhaps too frank in its revelation of Portuguese-Spanish rivalry. In contrast, the account by Jesuit Bartolomeu Guerreiro was approved for publication not only because it contained nothing contrary to the faith but also because it demonstrated "the singular beneficence of His Catholic Majesty toward the Portuguese crown and the corresponding willingness of that crown for any great service of His Majesty."<sup>80</sup>

The political motives in the treatment of New Christian complicity are more difficult to determine. The claim of Jewish treason in the fall of the city exculpated the Portuguese defenders for their desertion and also weakened the charge that Spain had not given proper attention to the defense of Portuguese colonies, a sensitive issue after the loss of Ormuz in 1622.<sup>81</sup> That the loss had been an embarrassment was later made clear by Philip IV's graceless treatment of the governor of Bahia, Diogo de Mendonça Furtado, upon his release from Dutch captivity.<sup>82</sup>

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suggested that Portuguese, like Flemish and Aragonese, should be given high positions in royal service. He informed the king that the Portuguese, loyal in their hearts, were "vassals worthy of great esteem," whose discontent was born of their love for the king. See Elliott and de la Peña, *Memoriales y cartas*, 1: 92.

<sup>80</sup> Guerreiro, *Jornada dos vassallos*, license granted on November 7, 1625.

<sup>81</sup> In the propaganda war that followed the Portuguese rebellion of 1640, pro-Habsburg authors often used the reconquest of Bahia as an example of Spain's sacrifice on behalf of Portugal (see BNM, Ms. 2372). This argument was somewhat disingenuous, since Spain had its own reasons to defend Brazil. Still, it has been made more recently by Juan Pérez de Tudela y Bueso, *Sobre la defensa hispana del Brasil contra los holandeses (1624-1640)* (Madrid, 1974). Pérez de Tudela apparently did not know of the existence of an earlier work that treated some of the same themes. See Schwartz, "Luso-Spanish Relations in Hapsburg Brazil," 33-48.

<sup>82</sup> Mendonça Furtado spent over two years in prison in Holland until he was freed in a prisoner exchange. He waited in Madrid for ten months without being granted an audience and then was imprisoned for fourteen months in Lisbon, where, because he was a knight of the Order of Aviz, he

The accusations against the New Christians at Bahia reiterated many of the charges that had been made against them during the years of intense debate over the general pardon and the weakening of restrictions. The codification of the charges in the official version of events may have been a way for Olivares to demonstrate that the crown had not fundamentally changed its policy in regard to the New Christians, that it considered them dangerous, even though it might be willing at times to make use of them for reasons of state. He already had other plans for New Christian capital, initiating in 1625 a project for a state-sponsored India Company, and in 1626 introducing Portuguese merchants into government financial contracts (*asientos*).<sup>83</sup> Soon after a new pardon and reduction of restrictions on New Christians was issued in 1627 (with the usual strongly negative reaction from the Inquisition), the government authorized the India Company and included New Christians on the board of directors.<sup>84</sup> Eventually, the willingness of Olivares to end the restrictions on New Christians and attract them and their capital back to Spain contributed to his unpopularity and his fall from power.<sup>85</sup>

The count-duke may have been playing a double game, manipulating an official version of events in the story of the fall and reconquest of Bahia, which seemed to support the Inquisition and the nobility, while at the same time turning increasingly to the New Christians. The negative view of the New Christians at Bahia in the "official" Spanish accounts came close on the heels of the Pragmatic Sanction of 1623, and the publication of Tamayo y Vargas's chronicle of the expedition in 1627 followed the plans for the "Union of Arms." At the same time that the story of the voyage was being widely diffused between 1625 and 1628, Olivares was involved in negotiations with New Christian financiers. Certainly, he had used the tactic before and was to do so later. In 1630, the Inquisition mounted its response to the influence of New Christians in Madrid. During that year, a distressed nine-year-old boy denounced his New Christian parents for flogging a crucifix in their Madrid home. Passions rose to a frenzy as the house of the accused was torn down by a mob of the faithful. As a result of this incident, a great auto-da-fé was held in 1632, and both Philip IV and the count-duke attended, in a public demonstration of support for the Holy Office and its task. During the 1630s, while Olivares sought to extend his protection to important New Christians, he rarely acted publicly on the behalf of individuals who fell afoul of the

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was tried by the Juiz dos Cavalheiros and the Mesa da Consciência. His performance had already been reviewed by the auditor of Don Fadrique's fleet. He was acquitted, and by 1630 his situation was finally resolved. See his petition in BL, Egerton 1133, fol. 344. Compare AGS, Secretarias provinciales 1582, fol. 131.

<sup>83</sup> The idea of an India Company had been discussed for years. See A. R. Disney, *Twilight of the Pepper Empire* (Cambridge, Mass., 1978), 71–85. On Olivares and the India Company, see James Boyajian, *Portuguese Bankers at the Court of Spain, 1626–1650* (New Brunswick, N.J., 1983), 20–21.

<sup>84</sup> Disney, *Twilight*, 90–93; Azevedo, *História*, 186–91. The Inquisition was especially upset by the crown's order to delay a planned auto-da-fé in Évora in 1627.

<sup>85</sup> The extent of Olivares's interest in this issue including his relations with practicing Jews is discussed in Jonathan I. Israel, "The Jews of Spanish North Africa, 1600–1669," *Transactions: Jewish Historical Society of England*, 26 (1979): 71–86; see also his "Spain and the Dutch Sephardim, 1609–1660," *Studia rosenthalia*, 12 (1978): 1–61.

Inquisition. Instead, he sought to balance public image with unannounced policies that served the interests of the state.<sup>86</sup>

Finally, the lack of an accusation of complicity in the principal Portuguese account remains unexplained. The Portuguese disliked New Christians no less than did the Spaniards, but no immediate political purpose could be served in Portugal by ascribing the city's fall to New Christians. Meneses did hint at the issue, but his preoccupation was with the military events and the Castilian-Portuguese rivalry. Vieira and the other Jesuits wrote without reference to the New Christians. Eventually, after the restoration, the Jesuits and especially Vieira became, like Olivares, advocates of pro-New Christian and mercantile policies that conflicted directly with the position of the Inquisition. To what extent such ideas may have influenced their earlier accounts is unclear; the Jesuit observations were written for the internal use of the order and had no particular propaganda purpose.

PORTUGAL, WHETHER UNDER HABSBURG CONTROL and the guidance of Olivares until 1640 or Bragança rule thereafter, was faced with a difficult choice that demonstrated the limits of state power. In the sixteenth century, Portugal had created a centralized bureaucracy, sometimes at the expense of the nobility and the municipalities, but, like many of the newly emerging absolutist states of Europe, the goal of the state had been the preservation of seigneurial society, not its overthrow. The very formation of the state, however, created disruption; it might choose, when necessary, to sponsor or favor new social forces such as the mercantile class. In Portugal, the accommodation of the aristocracy to the new state was in question principally during the decades between 1620 and 1670.

The crown needed the Portuguese nobility, but Olivares had few illusions about it. In his great memorial of 1624, although he believed the Portuguese nobles were basically loyal, he urged their careful and selective employment. The Portuguese nobility, he felt, was filled with more self-satisfaction and affectation than could be found anywhere.<sup>87</sup> Another Spanish observer of the time wrote, "they were so filled with vanity that they did not fit on the street."<sup>88</sup> Olivares made good use of talented and loyal Portuguese noblemen like Francisco de Melo in the Lowlands or Antônio de Brito in Catalonia, but the secret report on the Portuguese nobles prepared for the vicereine Margarita of Parma in 1633 left no doubt about the low esteem in which many were held.<sup>89</sup> Olivares's later comment about one of the pro-Bragança plotters, the marquis of Ferreira, that he was so stupid that he did not know where Valladolid was located, nor was he capable of learning, may reflect a more general attitude.<sup>90</sup>

For its part, while the nobility had many complaints against the crown in the 1630s, it did not support the various peasant uprisings in that decade nor did most

<sup>86</sup> Kamen, *Spanish Inquisition*, 219; Elliott, *Count-Duke of Olivares*, 450.

<sup>87</sup> Elliott and de la Peña, *Memoriales y cartas*, 1: 90.

<sup>88</sup> "Los Portugueses e sus costumbres," Biblioteca Pública de Évora, ciii/2-21, fol. 76.

<sup>89</sup> Biblioteca Pública de Évora, CV-219.

<sup>90</sup> Olivares to Virgilio de Malvezzi, March 10, 1641, in Elliott and de la Peña, *Memoriales y cartas*, 2: 203.





FIGURE 4: Padre Antônio Vieira, S.J. (1608–1697), the great missionary and preacher, political adviser to Dom João IV and defender of the New Christians. From a 1745 engraving reprinted in Charles R. Boxer, *The Portuguese Seabourne Empire, 1415–1825* (London, 1969), plate 12.

of the upper nobility immediately join Bragança in 1640.<sup>91</sup> Of the roughly sixty titled nobles in Portugal at that time, twenty-two had been created by Philip IV. Without a king or court in Portugal, there had been a “ruralization” of the Portuguese nobility under the Habsburgs, but evidence exists that this situation

<sup>91</sup> Morgan Broadhead, “The Nobreza Superior in the Restoration” (unpublished paper, 1972).

had, in fact, strengthened their seigneurial authority and share of revenues.<sup>92</sup> Certainly, many of the upper nobility were slow to join the Bragança cause, and about a quarter of the titled nobles remained on the Habsburg side after 1640. Those who supported the Bragança cause found ways of justifying their politics by ridiculing their former awards and claiming ironically that if Olivares could point to the titled nobles he had created in Portugal, it was to show the legal papers (*títulos*) of what he had sold, that he had promised habits in the Order of Christ with their emblazoned crosses because he wished to crucify them all, and that if the Portuguese gentlemen wished to kiss the king's hand, it was because they had nothing else to put in their mouths.<sup>93</sup> Those who remained in Portugal found that the new monarch needed their services as well. In Portugal, as elsewhere in Europe, the nobility retained its control of the state at the end of the century. While, in most of Europe, the nobility's position was no longer dominant, in Portugal it was close to being so.<sup>94</sup>

In the case of Portugal, a state with a small productive base, a lack of specie, and a considerable investment in its overseas trade and colonial products, the mercantile class had to figure in the crown's political and economic calculations in a central way. The defenders of the New Christians and the merchant class argued their utility to the state. "Commerce," declared Gómez Solís, is "more powerful than arms." To ignore the merchants was to court ruin. "Rich merchants are highly regarded everywhere in the world because they are the persons most useful to the republic," and this consideration, he maintained, should be even clearer in Portugal, dependent on the commerce of its conquests. But merchants were held in contempt by a nobility often in need of their aid. Gómez Solís argued that "if they hate some for being Jews, they hate others for being rich and powerful merchants."<sup>95</sup> This same position was echoed by González de Cellorigo, who called the merchants the "treasurers and foundation of kingdoms," pointing out that the international trading connections of the New Christian merchants were vital to Portugal's survival.<sup>96</sup> In fact, as Brazilian sugar, privately produced and marketed, surpassed the state monopoly on Indian Ocean spices in importance to the Portuguese economy, the role of the merchants became even more crucial.<sup>97</sup>

Olivares's attempt to assist the New Christian merchants and create an atmosphere favorable to their interests aimed ultimately at strengthening the state. It

<sup>92</sup> Hespanha, *As Vésperas*, 312–13. Hespanha's study is based on the distribution of offices, rents, and income in 1640, but he did not examine in detail the extent to which that distribution may have been the result of recent policies. On seigneurial privilege in general, see Michael Bush, *Noble Privilege* (New York, 1983), 144–85.

<sup>93</sup> *Memorial histórico español*, 16 (1862): 436–39. The statement is reported in Jesuit correspondence and is dated Madrid, July 30, 1642, but was probably written in Lisbon.

<sup>94</sup> I have discussed this more fully in Stuart B. Schwartz, *Sugar Plantations in the Formation of Brazilian Society* (Cambridge, 1985), 257–59; compare Rabb, *Struggle for Stability in Early Modern Europe*, 60–73.

<sup>95</sup> Duarte Gómez Solís, *Discursos sobre los comercios de las dos Indias* (n.p., 1622); see also his *Alegación en favor de la compañía de la India oriental*, Moses Bensabat Amzalak, ed. (Lisbon, 1955).

<sup>96</sup> Révah, "Le Plaidoyer," 390.

<sup>97</sup> Saraiva, *Inquisição*, 176. See also Meyer Kayserling, *História dos judeus em Portugal*, Anita Novinsky, ed. (São Paulo, 1971). The added notes and bibliography make this translation of the original German classic of 1867 once again important.

was a policy based not only on Spain's continued need for the "Portuguese" bankers, whose loans supported the crown even after the rebellion of Portugal but also on its need for increased commerce. Perhaps the Iberian New Christians could even be lured back from their exile.<sup>98</sup> This policy placed the needs of the state clearly before the ideological demands of religion as represented by the Inquisition and the social hierarchy defended by the nobility. On the Portuguese side, a small group of nobles, financial advisers, and Jesuits formed around Padre Antônio Vieira, João IV's adviser and confidant. He advocated a similar policy, realizing that the restoration could only be accomplished with money and that the economic well-being of Portugal ultimately depended on its commerce. He asked how a kingdom founded entirely on commerce could cast out its own merchants while admitting foreign ones.<sup>99</sup> In a testimony to his statesmanship, Vieira struggled for fair treatment of the New Christians and succeeded in convincing João IV to create a monopoly company for the Brazilian trade in which New Christians could invest without fear of losing their capital if accused by the Inquisition. Like Olivares, Vieira saw in New Christian capital and commercial ties the means to state power. As he put it, "having won Portugal from the Castilians, the King still had to storm the fortress of the Rossio [the palace of the Lisbon Inquisition]."<sup>100</sup>

But these pro-mercantile policies ran counter to the interests and beliefs of the Inquisition and the nobility in both Spain and Portugal. Charges of philo-Semitism and leniency toward New Christians and even Jews contributed to Olivares's loss of popularity and his fall from power in 1643. Vieira remained close to power throughout the reign of João IV and active thereafter, but the death of the king in 1656 and Vieira's brief arrest by the Inquisition in the 1660s impaired his later effectiveness. João IV, dependent on the nobility and on popular support, needed the ideological backing of the Holy Office and traditional society. No matter what his private opinion of the Inquisitors, João's public stance was often deferential, even when those New Christians who had been personal confidants and financial backers were arrested. Whether the Inquisition prosecuted New Christians because they were, in fact, secret Judiaizers or out of its own interest in continued power and in the profitable confiscations from the accused is a secondary issue in terms of the long-term effects of the policy: the destruction of any bourgeois faction with a hope of political power.<sup>101</sup> Still, in Portugal, the question of New Christian capital remained alive into the eighteenth century.

<sup>98</sup> On the replacement of Genoese financiers with Portuguese bankers, see Boyajian, *Portuguese Bankers*, 138–39; Antonio Domínguez Ortiz, *Política y hacienda de Felipe IV*, 2d edn. (Madrid, 1960), 121–47.

<sup>99</sup> "Proposta . . ." (1643), cited by Richard Graham, *Antônio Vieira and the Economic Rehabilitation of Portugal* (São Paulo, 1978), 90.

<sup>100</sup> Cited in Antônio José Saraiva, *Inquisição e cristãos novos*, 5th edn. (Lisbon, 1985), 192. There is lamentably little on Vieira in English. For an introduction, see C. R. Boxer, *A Great Luso-Brazilian Figure: Padre Antônio Vieira, S.J., 1608–1697* (London, 1957).

<sup>101</sup> I am using faction here as "a social group whose members have banded together in order to further their own best interests, and whose methods while undoubtedly opportunist, might be perfectly legitimate and legal." See Roger Mettam, *Power and Faction in Louis XIV's France* (London, 1987), 1.

Both Olivares and Vieira ultimately failed. Their pro-mercantilist policies conflicted too directly with an established order that, despite the growth of central governmental power, continued to determine the character of that power and its use. The crown sought, uncomfortably at times, to satisfy its practical needs without abandoning its ideological position within the traditional order. The attack on the New Christians was ultimately a critique of merchants in general, even those who by origins and orthodoxy were acceptable to the nobility and the Inquisition.<sup>102</sup> The struggle between bourgeoisie and nobility, masked and diffused in many early modern societies by the persistence of aristocratic ideals throughout society, was heightened and clarified in Portugal by the issues of religious orthodoxy and social intolerance.

<sup>102</sup> Smith, "Mercantile Class," 406–18. Smith pointed out that while the majority of the merchants were New Christians, many others were not. These, however, were infused with aristocratic ideals and tried to differentiate themselves from their New Christian colleagues by acceptance of those ideals. They thus proved economically conservative and unimaginative in their use of capital.

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## Inventing Siberia: Visions of the Russian East in the Early Nineteenth Century

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MARK BASSIN

Siberia is unknown in Europe and little-known in Russia.

Ippolit Zavalishin

My Russia, my life, need we suffer together?  
The tsar, and Siberia, and Ermak, and prison!  
O, isn't it time to part ways, to repent . . .  
What does a free spirit need with your darkness?

Alexander Blok<sup>1</sup>

THE POLITICAL EXPANSION OF EUROPE beyond its continental limits, which began with the acquisition of vast colonial empires in the sixteenth century, thrust a special sort of intellectual and psychological challenge upon the European consciousness: to represent to itself and interpret the nature of the exotic, remote regions of the globe with which it was suddenly and intimately confronted. The response to this challenge set in motion a creative process that continues to the present day and has borne rich fruit in the form of colorful and diverse geographical images or representations of the non-European world. The fanciful and often fantastic character of these images makes them an entertaining object for study, but their imaginative excesses should not obscure their deeper significance for the intellectual and indeed spiritual constitution of European civilization. While these images, as Henri Baudet has pointed out, were certainly not founded in the "perceptible reality" of the regions in question,<sup>2</sup> neither were they the result of a simple and random distribution of exotic characteristics and attributes across the world map. Rather, in each case, they represented the careful

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<sup>1</sup> I. Zavalishin, *Opisanie zapadnoi Sibiri* (Moscow, 1862), 1; A. A. Blok, "Rodina" (1910), *Sochineniia v odnom tome* (Moscow-Leningrad, 1946), 228.

<sup>2</sup> Henri Baudet, *Paradise on Earth: Some Thoughts on European Images of Non-European Man*, E. Wenholt, trans. (New Haven, Conn., 1965), 6.



(if unconsciously motivated) construction or invention, for a specific purpose, of an external world or geographical Other of a particular type. The identity assigned to these geographical externalities corresponded to the ideological categories of their progenitors, in other words, the constellation of beliefs and fears, predilections, prejudices, and needs of European society itself. Once created, these images entered this constellation and assumed their own special function within it as a discrete category.

The conceptual assimilation of the geographical Other introduced a dialectical tension into the European world view that determined how Europe perceived the outside world and, much more important, became virtually indispensable to its self-conception as well.<sup>3</sup> This final point is most significant and may be seen in two different respects. In his provocative study of European attitudes toward Asia, for example, Edward Said argued that by bringing into existence the conceptual category of the "Orient," Europe was merely fashioning an antipodal point of reference for itself, an "antitype," as he called it, against which it could clearly establish its own identity and justify its own arrogant sense of cultural and social superiority. Tzvetan Todorov made essentially the same point in regard to the discovery and conquest of the New World.<sup>4</sup> At the same time, the vision of the geographical Other could serve a very different function. For disaffected individuals and groups in Europe, it could offer a powerful representation of escape and perhaps even salvation. In the non-European world, a dimly perceived but nonetheless entirely plausible arena seemed available for the articulation of those exalted hopes and even utopian aspirations that could not be accommodated within the confining and frustrating reality of European society. In either of these functions, the geographical Other became inextricably enmeshed in the larger ideological web of the European imperial imagination.

These themes have been pursued in a substantial interdisciplinary literature, in which historians, geographers, literary scholars, sociologists, critical theorists, and art historians explore the European "Othering" of Asia and Africa, of the Americas, and recently of Australia as well.<sup>5</sup> There has, however, been no parallel

<sup>3</sup> For discussions of the psychological aspects of this process, see Albert Memmi, *The Colonizer and the Colonized*, H. Greedfeld, trans. (New York, 1965); O. Mannoni, *Prospero and Caliban: The Psychology of Colonization*, 2d edn., P. Powesland, trans. (New York, 1964); Johannes Fabian, *Time and the Other: How Anthropology Makes Its Object* (New York, 1983).

<sup>4</sup> "[C]'est bien la conquête de l'Amérique que annonce et fonde notre [European] identité présente"; Tzvetan Todorov, *La Conquête de l'Amérique: La Question de l'autre* (Paris, 1982), 14. Edward W. Said, "Orientalism Reconsidered," in Francis Barker, et al., eds., *Europe and Its Others*, 2 vols. (Colchester, Essex, 1985), 1: 17; Edward Said, *Orientalism* (New York, 1979), *passim*.

<sup>5</sup> Barker, *Europe and Its Others*; Baudet, *Paradise on Earth*; V. G. Kiernan, *Lords of the Human Kind: European Attitudes towards the Outside World in the Imperial Age* (Harmondsworth, 1972); Jonah Raskin, *The Mythology of Imperialism* (New York, 1971); Raymond Schwab, *The Oriental Renaissance: Europe's Rediscovery of India and the East 1680–1880*, G. Patterson-Black and V. Reinking, trans. (New York, 1984); Hugh Honour, *Chinoiserie: The Vision of Cathay* (London, 1961); John M. Steadman, *The Myth of Asia* (New York, 1969); Benita Parry, *Delusions and Discoveries: Studies on India in the British Imagination 1880–1930* (London, 1972); Allen J. Greenberger, *The British Image of India: A Study in the Literature of Imperialism 1880–1960* (London, 1969); Rashna B. Singh, *The Imperishable Empire: A Study of British Fiction on Asia* (Washington, D.C., 1988); Robin W. Winks and James R. Rush, eds., *Asia in Western Fiction* (Honolulu, 1990); Andrew L. March, *The Idea of China* (New York, 1974); Bernard Smith, *European Vision and the South Pacific*, 2d edn. (New Haven, 1986); O. H. K. Spate, "The Pacific as an Artefact," in Niel Gunson, ed., *The Changing Pacific: Essays in Honour of H. E. Mande* (Melbourne, 1978), 32–45; Harriet Guest, "Imagining the South Pacific," *Journal of Historical Geography*, 12 (1986):

examination of this process in regard to one of the oldest and largest colonies of all: the vast expanse of taiga, tundra, and steppe that extends eastward from the Ural mountains across northern Asia to the Pacific.<sup>6</sup> Siberia offered fertile ground for the creation of such images, for it was not only foreign but a virtual *terra incognita*, about which Russians west of the Urals throughout the nineteenth century possessed little positive information. This circumstance gave free rein to the imagination of those who undertook to speculate about the Russian east and enabled them to use it as a translucent geographical canvas for what has aptly been called the “externalization of their private vision.”<sup>7</sup>

To be sure, the fact that the Russians have traditionally entertained highly contrasting and volatile images of Siberia is generally appreciated. Siberians themselves, understandably sensitive on the issue, have been expressing their annoyance and dismay at this sort of fetishization since at least the middle of the nineteenth century, and a number of specialized studies have traced it through the centuries.<sup>8</sup> Beyond identifying these images, however, there has been no

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425–28; Philip D. Curtin, *The Image of Africa: British Ideas and Action 1780–1850* (Madison, Wis., 1964). For America, see Antonella Gerbi, *La disputa del Nuovo Mondo: Storia di una polemica 1750–1900* (Milan, 1955); Edmundo O’Gorman, *The Invention of America: An Inquiry into the Historical Nature of the New World and the Meaning of Its History* (Bloomington, Ind., 1961); D. Echeverría, *Mirage in the West: A History of the French Image of American Society to 1815* (Princeton, N.J., 1957); Gilbert Chinard, *L’Amérique et le rêve exotique dans la littérature au XVII<sup>e</sup> et XVIII<sup>e</sup> siècle* (Paris, 1913); Ray A. Billington, *Land of Savagery, Land of Promise: The European Image of the American Frontier in the Nineteenth Century* (New York, 1981); and the review by C. Vann Woodward, “Spaghetti West,” *New York Review of Books* (June 11, 1981): 33–35. For a study of the images of America portrayed in European art, see Hugh Honour, *The New Golden Land: European Images of America from the Discoveries to the Present Time* (London, 1975). The fact that even in our own day, Europe continues to construct images of the New World as an antitype and geographical Other is eloquently demonstrated in two recent essays on the “American scene” by leading European intellectuals: Umberto Eco, *Travels in Hyperreality: Essays*, W. Weaver, trans. (New York, 1986); Jean Baudrillard, *Amérique* (Paris, 1986). On the “discovery” of Australia, see Paul Carter’s fascinating study *The Road to Botany Bay: An Essay in Spatial History* (London, 1987).

<sup>6</sup> To be sure, the question of Russian images of the territories beyond the Urals—contemporary as well as historical—is attracting growing attention. For recent studies of Russian perceptions of other realms of the empire, see Seymour Becker, “The Muslim East in Nineteenth-Century Russian Popular Historiography,” *Central Asian Survey*, 5 (1986): 25–47; Susan Layton, “The Creation of an Imaginative Caucasian Geography,” *Slavic Review*, 45 (1986): 470–85.

<sup>7</sup> Hugh Ridley, *Images of Imperial Rule* (London, 1983), 4. On the special perceptual qualities of *terra incognita*, see John K. Wright, “*Terrae Incognitae*: The Place of Imagination in Geography,” *Annals of the Association of American Geographers*, 37 (1947): 1–15. This innovative piece is gathered along with other essays by Wright on the theme of regional images in the collection *Human Nature in Geography* (Cambridge, Mass., 1966). Also see David Lowenthal and Martyn J. Bowden, eds., *Geographies of the Mind: Essays in Historical Geosophy in Honor of John Kirtland Wright* (New York, 1976).

<sup>8</sup> In 1865, N. M. Iadrintsev noted with considerable bitterness: “Siberia has been either the object of exaggeration and praise, of joyous aspirations and dreams . . . and the most auspicious images, sometimes even taking on fairytale-like proportions; then suddenly, it [could] become the object of disappointment, blame, and betrayed hopes”; “Sibir’ pered sudom russkoi literatury,” *Literaturnoe nasledstvo Sibiri*, 5 (1980): 21. See especially N. M. Iadrintsev, *Sibir’ kak koloniia v geograficheskom, etnograficheskom, i istoricheskom otnoshenii*, 2d edn. (St. Petersburg, 1892); S. G. Svatikov, *Rossiiia i Sibir’ (K istorii sibirskogo oblastnichestva v XIX v.* (Prague, 1929); Mark Bassin, “Expansion and Colonialism on the Eastern Frontier: Views of Siberia and the Far East in Pre-Petrine Russia,” *Journal of Historical Geography*, 14 (1988): 3–21; V. G. Mirzoev, *Istoriografiia Sibiri (domarksistskii period)* (Moscow, 1970); L. M. Goriushkin and N. A. Minenko, *Istoriografiia Sibiri dooktiabr’skogo perioda: Konets XVI–nachalo XX v.* (Novosibirsk, 1984); E. A. Kuklina, et al., eds., *Voprosy russkoi i sovetskoi literatury Sibiri* (Novosibirsk, 1971); M. K. Azadovskii, “Poetika ‘giblogo mesta’ (Iz istorii sibirskogo peizazha v russkoi literature),” in Azadovskii, *Ocherki literatury i kul’tury Sibiri* (Irkutsk, 1947), 165–200. For bibliographies of Siberia

attempt to analyze them on a deeper level, to determine how they originated and were constructed, what spoken and unspoken assumptions they contained, which individuals or groups shared them, or why they differed. Consequently, no attempt has been made to fit these images back into the larger ideological complex that produced them and by so doing to determine what insights they might provide into the mind and culture of imperial Russia itself. It is to this latter problem that the present essay is dedicated.

One qualification may be made at the outset. Although Siberia was seen by many European Russians as a foreign Asiatic colony, it was at the same time somewhat more than this. The simple circumstance of territorial contiguity with the metropolis—a geographical arrangement shared by no other European empire—together with Siberia's large and long-established Russian population made it possible to see the territories beyond the Urals as a continuation or extension of the zone of Russian culture and society. This was an image of Siberia as Russia's frontier, and the best parallel was not European colonial domains but America's perceptions and myths of its own frontier, the Wild West.<sup>9</sup> Indeed, the American example first suggested to the Russians that they could see their eastern reaches in this light. Of course, in reading Siberia as their frontier, the Russians, like the Americans, were consciously reading themselves rather than a foreign realm. This point in turn ensured that, while the resulting images were no less contrived and fanciful, no less the product of their progenitors' inventive imagination than any others, they acquired a different dynamic and could serve different ideological functions.

The present essay is thus a first attempt to explore the usefulness of an analytical perspective that has not been widely applied in the Russian context. The time frame is the early nineteenth century. During this period, older traditional views of Siberia were both embellished and challenged by an array of new images and associations, which collectively suggested a different perspective on the Russian east. It is my intention to demonstrate that these multifarious images of Siberia were the product of the growing diversification and fragmentation of cultural and political thought in European Russia itself. Four discrete images or image clusters have been identified and will serve as the focus for analysis. These examples convey the extraordinary range and diversity that characterized Russian images of Siberia—from a desolate desert of snow and ice to Russia's promised land—and further serve to illustrate the complex ideological process by which the vision of a geographical Other was constructed and assigned a meaning.

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as a theme in Russian literature, see *Russkaia literatura Sibiri: Bibliograficheskii ukazatel'*, 2 vols. (Novosibirsk, 1976–77); M. K. Azadovskii, "Sibir' v russkoi khudozhestvennoi literature," *Izvestiia Vostochno-Sibirskogo Otdela Russkogo Geograficheskogo Obshchestva*, 51 (1926): 175–90.

<sup>9</sup> On American perceptions of their western frontier, see Richard Slotkin, *The Fatal Environment: The Myth of the Frontier in the Age of Industrialization 1800–1890* (Middletown, Conn., 1985); Richard Slotkin, *Regeneration through Violence: The Mythology of the American Frontier* (Middletown, 1973); Henry N. Smith, *Virgin Land: The American West as Symbol and Myth* (New York, 1957); Ray A. Billington and Albert Camarillo, *The American Southwest: Image and Reality* (Los Angeles, 1979).

THE VIEW OF SIBERIA THAT IN THE EARLY NINETEENTH CENTURY was probably shared most widely by Russians west of the Urals may be termed the colonial, or even better, imperial image. This was a picture of Siberia as a mercantile colony of the Russian state, for all intents and purposes a foreign territorial possession useful exclusively by virtue of the natural resources available there for exploitation. The colonial image derived naturally from the traditional relationship between Russia and Siberia, which from its inception was that of a metropolis to a colony.<sup>10</sup> In a manner resembling that of the Iberian empires in the New World, the Russians who crossed the Urals to “conquer” and then occupy Siberia did so in quest of an easily obtained and highly lucrative mercantile commodity, fur, which presented an alternative nearly as attractive as the precious metals of the New World. On domestic and foreign markets, the luscious pelts of the Siberian taiga were as precious as gold and silver, a fact underscored by the common Russian reference to them as “soft gold.” Over the centuries, furs from Siberia provided a steady and critically important source of income for medieval Russia.<sup>11</sup> During its heyday, the fiscal significance of the fur trade can hardly be overstated. One well-informed observer, the seventeenth-century official Grigorii Kotoshikhin, estimated that fully one-third of the total state revenues in the 1640s derived from Siberian pelts.<sup>12</sup>

The image of Siberia as a foreign mercantile colony rested logically on the larger vision of Russia as a colonial empire, and thus its formal articulation must be sought in the early eighteenth century, when the imperial idea was formalized. Immediately after the Russian victory in 1721 over Sweden in the Great Northern War, Peter the Great proclaimed the tsardom of Muscovy an *imperiiia*, or modern colonial empire, on the West European model. Like its Western counterparts, Russia was now to be identified formally as a geographical composite, made up of organically Russian regions representing the European metropolis and a vast, extra-European colonial domain. This picture of geographical symmetry was completed only after Peter’s death by one of his most brilliant ideologues, the

<sup>10</sup> For general histories of Siberia and Russian-Siberian relations, see *Istoriia Sibiri s drevneishikh vremen do nashikh dnei*, 5 vols. (Leningrad, 1968–69); V. K. Andrievich, *Istoricheskiĭ ocherk Sibiri po dannym predstavliaevym polnym sobraniiem zakonov*, 6 vols. (St. Petersburg, 1886–89); V. I. Ogorodnikov, *Ocherk istorii Sibiri do nachala XIX stoletii*, 2 vols. (Irkutsk, 1920–24); G. F. Miller, *Istoriia Sibiri*, 2 vols. (Moscow-Leningrad, 1937–41); A. I. Andreev, *Ocherki po istochnikovedeniiu Sibiri*, 2 vols. (Moscow-Leningrad, 1939–65); Iadrintsev, *Sibir’ kak koloniia*; G. V. Lantzeff, *Siberia in the Seventeenth Century: A Study of the Colonial Administration* (Berkeley, Calif., 1943); G. V. Lantzeff and R. A. Pierce, *Eastward to Empire: Exploration and Conquest on the Russian Open Frontier, to 1750* (Montreal, 1973); J. R. Gibson, “The Significance of Siberia to Tsarist Russia,” *Canadian Slavonic Papers*, 14 (1972): 442–53. For a study of the earliest Russian knowledge about and perceptions of Siberia, see D. N. Anuchin, “K istorii oznakomleniia s Sibir’iu do Ermaka,” *Drevnosti: Trudy Moskovskogo Arkheologicheskogo Obshchestva*, 14 (1890): 227–313.

<sup>11</sup> On the Siberian fur trade, see P. N. Pavlov, *Promyslovaia kolonizatsiia Sibiri v XVII veke* (Krasnoyarsk, 1974); R. H. Fisher, *The Russian Fur Trade, 1550–1700* (Berkeley, Calif., 1943); J. R. Gibson, *Feeding the Russian Fur Trade: Provisionment of the Okhotsk Seaboard and the Kamchatka Peninsula, 1639–1856* (Madison, Wis., 1969).

<sup>12</sup> A. V. Baikalov, “The Conquest and Colonization of Siberia,” *Slavic and East European Review*, 10 (1932): 561; G. Vernadskii, “Protiv solntsa: Rasprostranenie russkogo gosudarstva k vostoku,” *Russkaia Mysl’*, 35 (1914): 63; Gibson, “Significance of Siberia,” 443. Also see Bassin, “Expansion and Colonialism on the Eastern Frontier,” 11–15; Fisher, *Russian Fur Trade*, 17, 119–20.

geographer and historian Vasilii N. Tatishchev. In the 1730s, Tatishchev redefined for Russians the geographical boundary between Europe and Asia, shifting it from the commonly accepted Don River line, far to the east, to the Ural mountains.<sup>13</sup> The traditional boundary between Russia and Siberia thus became the demarcation between the civilization and culture of two world continents as well, and in one stroke Siberia was transformed into an Asiatic realm cleanly set off from a newly identified "European" Russia. A sophisticated array of contrasting geographical, cultural, and ethnographic characteristics and attributes were accordingly marshaled to embellish the distinction between the two. The product was an entirely novel self-image of Russia as a country divided into discrete "European" and "Asiatic" components, a geographical and geopolitical vision precisely corresponding to the categories and assertions of the new ideology.<sup>14</sup>

In this manner, Siberia was "Asianized" in the eyes of the Russians as an intrinsic part of the same conceptual metamorphosis by which Russia itself was westernized. The creation of this non-European geographical Other was essential to the latter process, for it was well appreciated that Siberia qua Asia represented a reliable and incontestable contrast that helped confirm the European identity of Russia west of the Urals. This critical function explained the emphasis, indeed, the insistence immediately placed on Siberia's non-European character, its dissimilarities with other parts of Russia, and the affinities it was assumed to share with the colonial possessions of other European empires. Throughout the remainder of the eighteenth century, Russian geography texts and atlases increasingly referred to the territories east of the Urals not by their native Russian name of Siberia but by the distinctly more foreign and exotic-sounding designation Great Tatar, which the Russians borrowed from the geographical terminology of the West.<sup>15</sup> Depictions of Siberia dwelled on the region's sparse but highly variegated indigenous population of Tungus, Yakuts, Eskimos, and other groups. The ethnographic and cultural contrast these peoples presented to their conquerors from Russia was depicted as identical to that of the Aztecs of Mexico or the "red men" of North America to the conquerors from Western Europe. These indigenous peoples served to characterize Siberia for imperial Russia, and the fact that the region also had a long-established population of ethnic Russians about equal in number was played down

<sup>13</sup> V. N. Tatishchev, *Leksikon rossiiskoi istoricheskoi, geograficheskoi, politicheskoi i grazhdanskoi* (St. Petersburg, 1793), 189; A. B. Ditmar, "K istorii voprosa o granitse mezhdru Evropoi i Aziei," *Uchenye zapiski Iaroslavskogo pedagogicheskogo instituta*, 30 (1958): 40–42; W. H. Parker, "Europe: How Far?" *Geographical Journal*, 126 (1960): 284–86.

<sup>14</sup> For a more detailed examination of this process, see Mark Bassin, "Russia between Europe and Asia: The Ideological Construction of Geographical Space" (*Slavic Review*, in press).

<sup>15</sup> Compare *Kratkoe rukovodstvo k geografii v pol'zu uchashchegosia pri gimnazii iunoshestva* (St. Petersburg, 1742), 58; M. I. Klevetskii, *Rukovodstvo k geografii s upotrebleniiem zemnogo shara i landkart* . . . , 3 vols. (St. Petersburg, 1773), 1: 106; L. A. Baumann, *Kratkoe nachertanie geografii dlia nachinaiushchikh obuchat'sia sei nauke* . . . , V. Ivanov, trans. (Moscow, 1788), 103–06; *Vvedenie v geografiiu, sluzhashchee ko iz'iasneniiu vsekh landkart, zemnogo shara s gosudarstvennymi gerbami, i opisaniie sfery* . . . , 2d edn. (Moscow, 1790), 256–57.





Yakuten (oben) und Buriaten. Kupferstich, 18. Jahrhundert

Siberia's "Red Men": Yakuts (above) and Buriats. German engraving, eighteenth century. Jeri Semjonow, *Die Eroberung Sibirens* (Berlin, 1937), facing p. 128.

or simply ignored.<sup>16</sup> The extent to which Siberia was understood within the framework of West European colonialism was indicated by the common practice of referring to it as “our Peru” or “our Mexico,” a “Russian Brazil,” or even “our East India,” a habit of mind that endured in Russian officialdom well into the nineteenth century.<sup>17</sup>

It should be noted that at the outset and for some time thereafter, this image of Siberia as a remote Asiatic colony was by no means a dim one. To the contrary, as part of the blossoming of Russia’s imperial regime under Catherine the Great (1762–1796), the colonial glory of Siberia reached its apogee. Comparing Siberian rivers such as the Lena to the Nile, for example, Mikhail Lomonosov dedicated odes to the splendid and rich natural environment of the country and, in the early 1760s, made his famous affirmation that “Siberia will foster the growth of Russian imperial grandeur.”<sup>18</sup> Catherine, for her part, entertained the vision of Siberia as a flourishing and self-sufficient colonial realm. For some years during her reign, a special Siberian currency was minted for exclusive use there, and it was apparently at this time that the characterization of Siberia as Russia’s *zolotoe dno* or “gold mine” became popular in European Russia.<sup>19</sup>

By the early nineteenth century, however, this Russian Peru had lost much of its economic significance. The fur trade had declined precipitously, a victim of eroded international markets, changing fashions of dress, and not least the decrease in available pelts, as the fur-bearing population of the Siberian mainland was hunted into virtual extinction. Trappers attempted to compensate by substituting sea-otter pelts harvested in the waters of the North Pacific for the fox, sable, and ermine of the Siberian taiga, but, while this produced a brief economic efflorescence in Russian Alaska, it was not ultimately successful.<sup>20</sup> Nor did the expansion of mining and metallurgy energetically undertaken by Peter the Great as part of his program to develop Russian manufacturing begin to match the

<sup>16</sup> V. N. Tatishchev, “Obshchee geograficheskoe opisanie vseia Sibiri,” *Izbrannye trudy po geografii Rossii* (Moscow, 1950), 70–72. A statistical handbook on Siberia prepared by the Ministry of Internal Affairs in 1810 devoted only 15 pages to the Russian population compared with 110 pages on its native peoples. *Statisticheskoe obozrenie Sibiri, sostavlennoe na osnovanii svedenii, pocherpnutykh iz aktov pravitel'stva i drugikh dostovernykh istochnikov* (St. Petersburg, 1810), 73–74, 75–200. For a discussion of the effects of the Petrine revolution on views of Siberia, see Yuri Slezkine, “Russia’s Small Peoples: The Policies and Attitudes towards the Native Northerners, Seventeenth Century–1938” (Ph.D. dissertation, University of Texas at Austin, 1989), 81–99.

<sup>17</sup> Perceptually, Siberia was “almost as separate [from European Russia] as if it were composed of overseas dependencies”; J. M. K. Vyvyan, “Russia in Europe and Asia,” *The New Cambridge Modern History* (Cambridge, 1960), 10: 38. For comparisons to Peru and other colonies, see *Statisticheskoe obozrenie Sibiri*, 1; V. P. Parshin, *Poezdka v zabaikal'skii krai*, 2 vols. (Moscow, 1844), 1: 32–33; Iadrintsev, *Sibir' kak koloniia*, 710; E. Petri, “Sibir' kak koloniia,” *Sibirskii Sbornik*, 2 (1886): 83; Svatikov, *Rossia i Sibir'*, 13.

<sup>18</sup> M. V. Lomonosov, “Kratkoe opisanie raznykh putesthestvii po severnym moriam i pokazanie vozmozhnogo prokhodu sibirskim okeanom v Vostochnuiu Indiiu” (1762–63), in Lomonosov, *Polnoe sobranie sochinenii*, 11 vols. (Moscow-Leningrad, 1950–83), 6: 498. For the comparison to the Nile, see “Oda na den' vosshestviia na vserossiiskii prestol Ee Velichestva Gosudaryni Imperatritsy Elisavety Petrovny 1747 goda” (1747), *Polnoe sobranie sochinenii*, 8: 203.

<sup>19</sup> Svatikov, *Rossia i Sibir'*, 6; D. W. Treadgold, *The Great Siberian Migration: Government and Peasant in Resettlement from Emancipation to the First World War* (Princeton, N.J., 1957), 20. [F. I. Soimonov], “Drevniaia poslovitsa: Sibir'—zolotoe dno,” *Ezhemesiachnye sochineniia i perevody, k pol'ze i uvnesleniiu*, 2 (November 1761): 449–67; Iadrintsev, “Sibir' pered sudom russkoi literatury,” 23.

<sup>20</sup> James R. Gibson, “Sables to Sea Otters: Russia Enters the Pacific,” *Alaska Review*, 3 (Fall–Winter 1968–69): 203–17.

relative economic importance furs had commanded, although it did replace the fur trade as Siberia's single most important economic enterprise.<sup>21</sup> Around 1800, as D. K. Fieldhouse noted, Siberia appeared to be repeating the pattern of colonial decline exemplified by the Portuguese possessions of Angola and Mozambique and represented for all practical purposes a "relic of an earlier period of Muscovite imperialism."<sup>22</sup> Indeed, it increasingly seemed that the only useful service Siberia continued to render was as a place of exile: an expansive storehouse for people whose presence in European Russia the authorities deemed to be socially or politically undesirable.

This waning of economic significance set a different tone for the imperial image of the Russian east. Enthusiasm for Siberia's potential gave way to an all-pervading cynicism about the value of the colony to its motherland. The generous vision of Siberia as a "gold mine" was gone for the Russian government as well as for much of the educated Russian public, replaced by the menacing picture of a vast Asiatic wasteland of barren, snowy expanses and frozen tundra. In the absence of a demonstrable mercantile value, Siberia's negative qualities as an antitype to European Russia became definitive. To the Russians of the early nineteenth century, who still looked unquestioningly on Western Europe as the ultimate model of the advanced and enlightened civilization they desired for themselves, Siberia presented a distinctly disagreeable, even ominous, prospect. It epitomized the primitiveness and lack of Western development they found contemptible, and it consequently excited a palpable disdain, an antipathy amplified by the burgeoning sense of insecurity on the part of Russians west of the Urals about the authenticity of their own claim to a European identity. A tsarist bureaucrat captured the essence of this pervasive attitude with the smug observation that "Nevskii Prospect alone is worth at least five times as much as all of Siberia," and the exaggeration of the comparison only served to underscore the high importance the Russians attached to this point.<sup>23</sup> The dynamism of St. Petersburg's premier avenue, with its high-fashion salons, newspaper kiosks, Western-style architecture, stock exchanges, and libraries offered the best evidence that Russia was indeed an enlightened and advanced European society. As such, it represented the antithesis of inhospitable, desolate, and uncivilized Siberia.

For many Russians, the primeval aspect of untouched Siberia was frightening. Here was an immense yet empty stretch of territory, all the more awesome in that its extraordinary size was matched by the uselessness to which nature, it seemed, had condemned it. Its harsh physical environment apparently precluded settlement and productive exploitation. It was, moreover, completely isolated, cut off by high mountain ranges from any contact with the populous Asian societies to the south and access to the waters of the Pacific on the east. Its rivers, true enough, were spectacular, and among the mightiest in the world, but useless for the constructive purposes of commercial and passenger transport, for they flowed in the wrong direction and emptied into dark and frozen bays along the Arctic coast.

<sup>21</sup> A. S. Kuznetsov, "Sibirskaiia programma tsarizma 1852 g.," *Ocherki istorii Sibiri*, 2 (1971): 4–5, 15.

<sup>22</sup> D. K. Fieldhouse, *Economics and Empire* (Ithaca, N.Y., 1959), 159.

<sup>23</sup> Quoted in A. I. Stepanchenko, *Gordost' nasha Sibir': Molodym o zavetnom krae* (Irkutsk, 1964), 5.

An article in the popular journal *Notes of the Fatherland* presented a litany of these points in 1841. After a detailed discussion of Siberia from all aspects, including extended comment on the “terrible stagnation of its Asiatic climate” that condemned its inhabitants to eternal “intellectual stagnation,” the author concluded with the following grim observation: “In a word, whatever aspect of Siberia is examined—its climate, geographical position, physical make-up, its industry, current conditions of commerce, the half-wild nature of the countries bordering on it to the south, the inadequate road systems—all this forces one to think that it will long be fated to remain a desert.” Russia would be better off, he asserted, if the “ocean of snow” that was Siberia could be replaced by a real body of water, which would at least enable convenient maritime trade with the Far East.<sup>24</sup>

As a sort of complement to this picture of economic uselessness and even parasitism in the popular imagination—our author spoke of Siberia “sucking the juices out of Russia”—Siberia epitomized all that was negative.<sup>25</sup> This was a time, one contemporary observed, when the very mention of Siberia produced “sorrowful feelings as a domain of eternal wind storms and snow . . . , a dank and barren land, with gloomy penal mines.”<sup>26</sup> The poet P. V. Shumakher spent only one year in government service in Siberia during the mid-1830s but was nevertheless moved to commemorate his experience in the following verse:

O you, bitterness, cruel stepmother, Siberia,  
Your snowy steppes have spread out far and wide:  
Unfriendly, unfree, deserted, hostile,  
Unappealing, inhospitable, and cold.<sup>27</sup>

Shumakher's designation of Siberia as unfree referred to the most graphic, and by all means most enduring, of the negative associations evoked by the region: its function as a place of exile. Siberia's perceived severe natural conditions—its “stagnant Asiatic environment”—seemed entirely fitting to its function as an immense prison, and the two negative images were woven together and became mutually enhancing. This particular image even inspired a motif in the literature of Russian Romanticism in the 1820s and 1830s, the so-called poetic formula of Siberia. This formula involved a contrasting interplay between the wretched and inhuman natural environment of Siberia on the one hand and the exalted nobility and selfless sufferings of a hero-protagonist situated there on the other.<sup>28</sup> Its originator, Kondratii Ryleev, supplied an excellent example in an epic poem dedicated to the Ukrainian patriot Andrei Voinarovskii. Voinarovskii's tragic

<sup>24</sup> [N. M. ?] Gersevanov, “Zamechaniia o torgovykh otnosheniiakh Sibiri k Rossii,” *Otechestvennye zapiski*, 14 (1841), otd. 4: 33 (quote), 30, 26–27. Also see N. N. Koz'min, “M. B. Zagoskin i ego znachenie v istorii razvitiia sibirskoi obshchestvennosti,” in Koz'min, *Ocherki proshlogo i nastoiashchego Sibiri* (St. Petersburg, 1910), 173.

<sup>25</sup> Gersevanov, “Zamechaniia o torgovykh otnosheniiakh,” 30.

<sup>26</sup> I. D. Bul'chev, *Puteshestvie po vostochnoi Sibiri: Iakutskaiia Oblast', Okhotskii krai* (St. Petersburg, 1856), 1; M. M. Gedenshtrom, *Otryuki o Sibiri* (St. Petersburg, 1830), 4.

<sup>27</sup> P. V. Shumakher, “Pesnia katorzhnogo” (1862), in Shumakher, *Stikhi i pesni* (Moscow, 1902), 123.

<sup>28</sup> N. N. Kurdina, “U istokov poetiki sibirskogo peizazha v russkom romantizme,” in L. P. Iakimova, et al., eds., *Literatura Sibiri: Istoriia i sovremennost'* (Novosibirsk, 1984), 21 and following. Also see Azadovskii, “Poetika ‘giblogo mesta.’”





A convoy of exiles in western Siberia. George Kennan, *Siberia and the Exile System* (New York, 1891), vol. 1, p. 377.



demise in exile to eastern Siberia served for the future Decembrist as a model of a life sacrificed to the struggle for freedom and justice in the face of the most adverse conditions. Ryleev left no doubt that the region of exile itself figured as one of these adversities. The poem, written early in 1825, began with the following description of Yakutia and the Lena River:

The gloomy nature of these lands  
Is always harsh and wild,  
The angry river roars,  
Storms often rage,  
And the clouds are often dismal . . .

Fearing the winters  
Both interminable, and icy,  
No one will visit  
This wretched country,  
This vast prison house for the exiles.<sup>29</sup>

This was a far cry from Lomonosov's reference to the same river as a veritable Russian Nile. The image of Siberia as a place of exile became so firmly fixed in the Russian imagination at this time that the very name *Sibir'* (or its diminutive *Sibirka*) became a popular synonym for exile or penal servitude of any sort, irrespective of whether it actually took place in Siberia or not.<sup>30</sup>

The Siberian policy pursued by the Russian government reflected this negative view. With the decline of the fur trade, Siberia had become Russia's outback, and in the eyes of the government it should remain as such. This attitude was especially pronounced during the reign of Nicholas I (1825–1855), whose preoccupation with European affairs left him little time for the eastern periphery of the empire. The observation of his foreign minister Count Karl Nesselrode that Siberia's most useful function was as a "deep net" for criminals and other dregs of Russian society demonstrated how substantially official views had changed in little over half a century.<sup>31</sup> The government's express lack of interest in the development of Siberia reflected certain pragmatic considerations as well. Most immedi-

<sup>29</sup> K. F. Ryleev, "Voinarovskii," *Polnoe sobranie sochinenii* (Leningrad, 1971), 192.

<sup>30</sup> Iu. M. Lotman, "Dekabrist v povsednevnoi zhizni (bytovoe povedenie kak istoriko-psikhologicheskaiia kategoriia)," *Literaturnoe nasledie dekabristov* (Leningrad, 1975), 40. Other colloquial Russian words derived from the name Siberia, such as *sibirnyi* (savage, cruel, or evil) or *sibirshchina* (a difficult, burdensome, and unendurable life), give some indication of the extent to which attitudes on less literate levels corresponded to these negative views. Vladimir Dal', *Tolkovyi slovar' zhivogo velikorusskogo iazyka*, 4th edn., 4 vols. (St. Petersburg, 1912), 4: 143–44.

<sup>31</sup> Cited in I. P. Barsukov, ed., *Graf Nikolai Nikolaevich Murav'ev-Amurskii po ego pis'mam . . .*, 2 vols. (Moscow, 1891), 1: 670. On governmental attitudes and policy toward Siberia under Nicholas I, see Kuznetsov, "Sibirskaiia programma tsarizma," 17–18; G. P. Shatrova, *Dekabristy i Sibir'* (Tomsk, 1962), 118. Under Alexander I, there remained a vestige of enthusiasm among officials for Siberia's colonial potential, notably that of M. M. Speranskii, who prepared an administrative reform of the region in the early 1820s, but this attitude had largely vanished by the second quarter of the century. Marc Raeff, *Michael Speransky: Statesman of Imperial Russia*, 2d edn. (The Hague, 1969), 259. For other positive appraisals, see *Statisticheskoe obozrenie Sibiri*, 1 and following; A. K. Kornilov, *Zamechaniia o Sibiri* (St. Petersburg, 1828), 1; K. Arsen'ev, *Statisticheskii ocherk Rossii* (St. Petersburg, 1848), 25. Grigorii Spaskii's journal *Sibirskii Vestnik* (St. Petersburg, 1818–24) and its short-lived successor *Aziatskii Vestnik* (1825–27) also tried to keep alive the image of Siberia as a "gold mine."

ately disturbing was the fear that the emergence of an active, dynamic economic and civil life in Siberia might well lead the region to dissolve its ties to European Russia and proclaim independence. The specter of Siberian separatism was a terrifying one for officials in European Russia. Ironically, the comparison made by these officials themselves between Siberia and the New World colonies of the West European empires gave it substance, for, by the beginning of Nicholas I's reign, most of the New World colonies had achieved independence.<sup>32</sup> Officials may have perceived little economic use for the territories beyond the Urals, but they considered the idea of dissolving the empire through the breaking away of its greater half inadmissible. As late as 1861, Baron P. K. Meyendorf, head of the "Siberian Committee," which oversaw governmental policy beyond the Urals, vigorously opposed the economic development of Siberia. He argued that the repercussions would be intolerably dangerous, for the government risked its eventual "separation from the metropolis, as the history of all colonies teaches."<sup>33</sup>

THE CRYSTALLIZATION OF AN ARTICULATE AND SELF-CONSCIOUS political opposition in Russia in the late eighteenth and early nineteenth centuries gave rise to a perspective on Siberia fundamentally different from the negative colonial views just described. While the emergence of this new view was not unrelated to the intimate acquaintance that many of the early opponents of tsarist autocracy had with Siberia through long periods of exile there, exiled Russians had been learning about Siberia firsthand for generations without producing any significant perceptual transformation. More important to the shift was the fact that the unorthodox convictions of this opposition concerning the needs of Russian society and their disaffection from the country's autocratic status quo prepared them psychologically to appreciate the Russian east in an unprecedented manner. Their deep concern with the reform and reorganization of Russian society ensured that they would be open to new and unorthodox ideas, and in fact they actively sought out alternative social and political models for the transformation of Russia's medieval order. In this quest, Siberia proved to possess a special attraction. While it continued to function as a geographical Other, the specific ideological quality of the traditional relationship was inverted, and, rather than supplying a negative affirmation of the positive qualities of the European metropolis, it came to represent a radical alternative in which the much-sought-after progressive models could be identified. Thus the perceptual pendulum

<sup>32</sup> Kuznetsov, "Sibirskaiia programma tsarizma," 6–9, 11–12, 13, 25; Svatikov, *Rossiiia i Sibir'*, 15. Argentina gained independence in 1810, Venezuela in 1811, Chile in 1818, Mexico and Peru in 1821, and Brazil and Ecuador in 1822. Government officials in Siberia were well aware of St. Petersburg's extraordinary concern with separatist tendencies and played on these fears effectively in their communications with the capital, often toward contradictory purposes. The specter of Siberian separatism, for example, was used by the governor-general of eastern Siberia, N. N. Murav'ev, in the 1850s as a compelling reason to annex the Amur River but by his west Siberian counterpart as an irrefutable argument against annexation. Mark Bassin, "A Russian Mississippi? A Political-Geographical Inquiry into the Vision of Russia on the Pacific 1840–1865" (Ph.D. dissertation, University of California, Berkeley, 1983), 127, 141–42 and following.

<sup>33</sup> Quoted in S. G. Svatikov, "Rossiiia i Sibir'," *Vol'naiia Sibir'* (Prague), 8 (1930): 38.

swung, and, from a barren realm of ice and darkness, Siberia was transformed into an abiding source of hope and inspiration.

This new perspective came into full relief after 1825, articulated by the young officers banished to Siberia for their complicity in the revolt against the accession of Nicholas I in December of that year. The "Decembrists" spent decades beyond the Urals, and Siberia figured as an important theme in all their writings from this period. In the mournful poetry written during their early years of exile, the "Siberian formula" received what was probably its most powerful and moving expression.<sup>34</sup> At the same time, however, the use of this literary device did not prevent them from evaluating Siberia in a positive, even enthusiastic, light. They were much impressed by the region's untapped natural resources, the abundance and richness of which they could evaluate firsthand. But their primary interest was attracted by a different factor, one that had previously remained almost entirely unnoted: the nature of Siberian society itself. Up to this point, Siberia had been pictured as a region either devoid for all practical purposes of human population or as populated by a colorful but sparse and not particularly significant assortment of native Asian peoples. If the existence of a Russian population was acknowledged at all, it was generally depicted as a paltry assemblage of criminal offenders, troublesome religious schismatics, and simple wanderers (*guliashchie liudi*), groups that represented another one of Siberia's numerous inadequacies.<sup>35</sup> Yet it was precisely on Siberia's Russian society that the Decembrists focused their attention. They further broke with tradition by identifying in it a variety of qualities that contrasted favorably with Russia west of the Urals.

The most positive quality was the absence of the hated institution of serfdom, which had not migrated with the Russians across the Urals.<sup>36</sup> This circumstance had profound implications for the nature of society there, for it meant that Siberia was free of both chattel labor and a powerful landed aristocracy. Siberian society was consequently more democratic and egalitarian than European Russia and not fragmented into a rigid hierarchy of social classes. Nikolai Basargin described how his initial dread at the prospect of exile in Siberia ("I had ceased to consider myself an inhabitant of this world") was transformed once he arrived there and was able to see what the region was really like: "The further we traveled across Siberia, the more it gained in my estimation. The common people seemed to me to be much freer, cleverer, and even more highly educated than our Russian peasants, especially the serfs. The Siberians better understood the dignity of man, and valued their rights more highly."<sup>37</sup> East of the Urals, many of the exiled Decembrists discovered some of the very qualities considered most necessary for the progressive advance of European Russia itself. Siberia served as a counter-example to confirm the debilitating effects of serfdom on European Russia and at the same time demonstrated that Russian society, if freed from serfdom, was capable of recognizing and respecting fundamental human rights.

Like the government officials noted above, the Decembrists also drew a parallel

<sup>34</sup> Iu. S. Postnov, *Russkaia literatura Sibiri pervoi polviny XIX v.* (Novosibirsk, 1970), 117–32; Iu. S. Postnov, *Sibir v poezii dekabristov* (Novosibirsk, 1976).

<sup>35</sup> See, for example, Gersevanov, "Zamechaniia o torgovykh otnosheniiakh," 27–28.

<sup>36</sup> Shatrova, *Dekabristy i Sibir'*, 85.

<sup>37</sup> N. V. Basargin, *Zapiski* (Petrograd, 1917), 94.

between Siberia and the European colonies of the New World. The affinities they noted, however, were not with the South and Central American dominions of the Iberian empires but instead with the former colony of West European settlement to the north, the United States. Attitudes in Russia toward the young North American republic varied considerably, and its detractors were certainly as numerous as its supporters, if not more so. To advocates of political liberalization and reform, however, the United States stood as a powerful inspirational symbol, and the Decembrists were among its most faithful admirers.<sup>38</sup> The view they shared of the New World republic was little short of idyllic. Here was a country that had waged a successful war of liberation against colonial oppression and reconstituted itself as an independent political unit founded squarely on the virtuous principles of full personal liberty and constitutional democracy. The American national ethos, as they understood it, was characterized by an elemental egalitarianism that ignored inherited rank or class and emphasized instead the dignity and worth of each individual. Moreover, they perceived in American society a remarkable economic dynamism, fed by an apparently inexhaustible abundance of available fertile land and natural riches, which ensured that all honest enterprise and effort on the part of its citizens would be justly and amply rewarded.

This vision of America enabled the Decembrists to develop a new perspective on the Russian east, for the image of Siberia they ultimately formulated was essentially constructed out of categories borrowed from the North American model. In effect, it amounted to nothing less than the recognition of a parallel "second New World" in their own back yard, with the associated belief that Siberia could and would develop into the sort of society America already was.<sup>39</sup> In every important respect, Siberia seemed to them to possess the rudiments for a vigorous and thriving democracy. Siberia, like America, was a young and developing society, equally egalitarian, heedless of class distinctions, and generally unburdened by the accumulated inequities of traditional Russian society. Moreover, Siberia could draw on the same reserves of untapped natural wealth and abundant open lands to support its rapid, progressive development. The Decembrists were conscious of the extent to which the American model informed their views: "I regard Siberia no differently than the United States," declared Ivan Pushchin, and Basargin described the great similarities between Siberians and Americans in terms of their disposition, habits, and way of life. In regard to the question of human rights, he even ventured to place Siberia ahead of the United States, which, after all, still supported the institution of slavery.<sup>40</sup>

<sup>38</sup> "To the Russian radicals, children of the Enlightenment but also fettered by the absolutist authoritarian traditions of the Russian state and church, America was the distant fulfillment of that West which had given them the visions of active participation, the gospel of the dignity of labor, and the revolutionary expectation of a new society and a new man"; Hans Kohn, "Introduction," in Taras Hunczak, ed., *Russian Imperialism from Ivan the Great to the Revolution* (New Brunswick, N.J., 1974), 15; also see Max M. Laserson, *The American Impact on Russia—Diplomatic and Ideological: 1784–1917* (New York, 1950), 139.

<sup>39</sup> The first explicit identification of Siberia as "second New World" appears to have been made by the historian Nikolai Karamzin, *Istoriia gosudarstva rossiiskogo* (1816–29), 12 vols. (St. Petersburg, 1892), rpt. in *Slavistic Printings and Reprintings* (The Hague, 1969), 9: 232–33.

<sup>40</sup> I. I. Pushchin, *Zapiski o Pushchine i pis'ma iz Sibiri* (Moscow, 1925), 196–97; Basargin, *Zapiski*, 94, 178.

On a somewhat more abstract level, the Decembrists perceived a very important similarity in the potential represented by both regions as vast, untouched reservoirs, offering great scope of opportunity for individual betterment and enrichment, as well as for social progress. Mikhail Bestuzhev wrote back to European Russia in 1837:

Everything that you used to read in novels about America, for which people set off to improve their lot in life, and from which they returned as rich uncles to leave the fortunes they made there to their ne'er-do-well nephews, and where still today thousands are migrating from Europe to find a piece of land or to take part in America's active industrial life and with this earn their daily bread—all of this can be applied in the fullest sense to Siberia, with but one difference. Everyone is going to America, but they are all afraid to go to Siberia. In their mind, it is a savage land of exile, a kingdom of frost inhabited only by bears and bandits.<sup>41</sup>

Taken to one possible logical conclusion, the parallel with the United States referred not merely to affinities between the two societies but to an essential similarity in the nature of their relationship to Europe as well. It suggested the vision of Siberia as a land of the future able to perform the same function of salvation that North America had for the European Old World. As both a repository of land and resources and a youthful, energetic society unfettered by the bonds of oppressive feudal tradition and institutions, Siberia promised to realize the failed dreams of an exhausted European Russia west of the Urals. Returning from Siberian exile in 1837, Baron Andrei Rosen gave voice to these sentiments and speculated that "Siberia is perhaps destined to fill the role of North America, where people were banished, voluntarily or against their will, for their political or religious views, and where, by prayer and by hard work, they created in the new world all of those blessings which the old and experienced world has been seeking in vain for so long."<sup>42</sup>

There was, however, another logical conclusion to the comparison of Siberia and America that the Decembrists pointedly failed to draw. While the successful struggle of the United States for freedom and self-determination constituted a noble and inspiring symbol for them, they gave no indication that they saw it as an example their own Siberian territories should emulate.<sup>43</sup> As is clear from the various plans for the political reorganization of Russia they drafted prior to 1825, despite the radical changes called for in the country's political and social order, the Decembrists remained faithful to the vision of it as an imperial entity.<sup>44</sup> Even after the experience of exile beyond the Urals, which produced a dramatic transformation in their view of Siberia itself, their deeper conviction that the geopolitical unity and integrity of the empire ought to be maintained—even expanded—remained intact. In regard to the question of separation, therefore,

<sup>41</sup> Quoted in A. V. Gurevich, ed., *Vostochnaia Sibir' v rannei khudozhestvennoi proze* (Irkutsk, 1938), xi.

<sup>42</sup> A. E. Rozen, *Zapiski Dekabrista* (St. Petersburg, 1917), 213.

<sup>43</sup> Shatrova, *Dekabristy i Sibir'*, 88–94.

<sup>44</sup> Compare P. I. Pestel', *Russkaia pravda: Nakaz vremennomu verkhovnomu pravleniiu* (St. Petersburg, 1906), 13–17; "Proekt konstitutsii Nikity Murav'eva," in V. E. Iakushin, *Gosudarstvennaia vlast' i proekty gosudarstvennoi reformy v Rossii* (St. Petersburg, 1906), 136; "Proekt konstitutsii . . . kn. S. P. Trubetskogo," in M. V. Dovnar-Zapol'skii, ed., *Memuary dekabristov* (Kiev, 1906), 102–03. Also see *Vosstanie dekabristov: Materialy*, vol. 5 (Moscow, 1926), 126–27.



the Decembrists continued with few exceptions to perceive the region as a legitimate colony of Russia that required not independence but a modern, constructive colonial administration, directed at the rapid settlement and economic development of the region, and a more effective integration into the overall imperial structure.

THE STRONG REACTION TO THE COURSE OF EUROPEANIZATION initiated by Peter the Great produced yet a third cluster of images of Siberia. This reaction intensified during the reign of Nicholas I and took the ideological form of Russian nationalism. As in other European countries, nationalism in Russia began as a many-sided intellectual movement, which owed its cohesion to the common subscription of its adherents to certain basic principles. One of these was the demand for complete independence (*samostoiatel'nost'*) in all aspects of national life, a concern that immediately raised the question of Russia's relationship to Europe. With ever-greater passion, the nationalists argued that it was precisely the obsession with the West and the desire to set Russia as much as possible into a European mold that was obstructing them from recognizing their own unique identity and pursuing independently their own national destiny. They judged the much-belabored attempt to become European through adopting Western culture and Western attitudes a miserable failure that had yielded nothing more than shabby and contrived imitations. Russians must stop trying to emulate the West, the nationalists insisted, and should instead direct their attention and energies inward, toward the ultimate goal of complete self-awareness (*samopoznanie*). They hoped that this process of self-scrutiny would create a sense of Russian national exclusivity independent of Europe, for, through it, the Russians would learn to recognize and appreciate the national treasures that were theirs and theirs alone and to seek their inspiration and values in Russia's own historical legacy, its social institutions, and unique national ethos.<sup>45</sup>

Out of this process of self-examination, the nationalists discovered a variety of meanings for the Russian east. One of the most important was represented by the historical legacy of Russian expansion beyond the Urals. The new interest in Siberian history stemmed from a more general resurgence of interest in all aspects of Russian history, to which the nationalists turned in their quest for evidence of a national dynamism, vitality, and, above all, a capacity for independent action and creative achievement. In the chronicles that recounted the daring exploits of the Cossacks and fur traders who first crossed the Urals, conquered Siberia from the remnants of the Golden Horde, and displayed extraordinary endurance and initiative in traversing, settling, and exploiting these wild domains, the Russians found that they had a wealth of inspiring material. Moreover, the

<sup>45</sup> On the development of Russian nationalism during the reign of Nicholas I, see especially Nicholas V. Riasanovsky, *Nicholas I and Official Nationality in Russia, 1825–1855* (Berkeley, Calif., 1969); Martin Malia, *Alexander Herzen and the Birth of Russian Socialism* (New York, 1965); Edward C. Thaden, *Conservative Nationalism in Nineteenth-Century Russia* (Seattle, 1964); A. E. Presniakov, *Emperor Nicholas I of Russia: The Apogee of Autocracy 1825–1855*, J. C. Zacek, trans. (Gulf Breeze, Fla., 1974); Hans Kohn, *The Mind of Modern Russia: Historical and Political Thought of Russia's Great Age* (New Brunswick, N.J., 1955).

saga of the conquest of Siberia provided them with a reassuring counterbalance to the accomplishments of the Western empires in the New World. Europe may have earned its place in the annals of world history through the feats of the conquistadores, but, for the nationalists, the record of the occupation of Siberia demonstrated brilliantly that the Russians had been no less valiant or effective in their own exploits. In the figures of such Cossack heroes as the great Ermak, Erofei Khabarov, and Vladimir Atlasov, they could point with pride to their own native Cortes or Pizarro and did not hesitate to draw this comparison.<sup>46</sup> "We did not discover America," wrote the historian Mikhail Pogodin in a letter to the tsarevich Alexander in 1837, "but we opened up a third of Asia, . . . and doesn't [this] supplement Columbus's discovery?"<sup>47</sup>

This new appreciation of Siberian history called for a new historiography. The existing histories of the region, based largely on the research of the remarkable eighteenth-century German scholar Gerhard Müller for the Academy of Sciences in St. Petersburg, had depicted Ermak and the other Siberian Cossack conquerors negatively, as the obstreperous leaders of lawless bands of brigands interested only in their own gain.<sup>48</sup> These works consequently offered little encouragement for the new nationalist enthusiasm and were superseded in the 1830s, when a revisionist historiography of Siberia written exclusively by Russians began to appear.<sup>49</sup> In this new work, the conquerors of the Russian east were depicted in a manner more appropriate to nationalist sensibilities, as courageous and deeply patriotic sons of Russia whose deeds were animated by the overriding desire to enhance the glory of the fatherland.<sup>50</sup> Indeed, the nationalist exuberance of one historian led him to reject emphatically the suggestion of a parallel between Cortes and Ermak, with the argument that Ermak's accomplishments so surpassed those of the Spaniard that any comparison was implicitly denigrating for the Russians.<sup>51</sup>

The conquest of Siberia as part of Russia's national saga was further popular-

<sup>46</sup> See Nikolai Shchukin, "Podvigi russkikh na Amure v XVII stoletii . . .," *Syn otechestva*, Kn. 9, otd. 1 (1848), 27–28, 52; Seymour Becker, "Contributions to a Nationalist Ideology: Histories of Russia in the First Half of the Nineteenth Century," *Russian History/Histoire russe*, 13 (1986): 331–53.

<sup>47</sup> N. P. Barsukov, *Zhizn' i trudy M. P. Pogodina*, 22 vols. (St. Petersburg, 1888–1910), 5: 171.

<sup>48</sup> Müller referred to Ermak contemptuously as "nothing more than a fugitive cossack . . . and chief of a troop of banditti"; G. F. Müller, *Conquest of Siberia, and the History of the Transactions, Wars, Commerce, etc. etc. carried on between Russia and China* (London, 1842), 9. Also see G. Müller [Gerhard Müller], *Opisanie sibirskogo tsarstva i vsekh proishshedshikh v nem del . . .*, V. Lebedev and I. Golubtsov, trans. (St. Petersburg, 1750), republished in part in the late 1930s as *Istoriia Sibiri*; Johann Eberhart Fischer, *Sibirische Geschichte von der Entdeckung Sibiriens bis auf die Eroberung dieses Landes durch die russische Waffen*, 2 vols. (St. Petersburg, 1768); Mirzoev, *Istoriografiia Sibiri*, 87–88, 94. On Müller, see J. Lawrence Black, *G.-F. Müller and the Imperial Russian Academy* (Kingston, Ont., 1986).

<sup>49</sup> P. A. Slovtsov, *Istoricheskoe obozrenie Sibiri*, 2 vols. (St. Petersburg, 1838–44); P. I. Nebol'sin, *Pokorenie Sibiri: Istoricheskoe issledovanie* (St. Petersburg, 1849). For recent discussions of the historiography of Ermak's campaign, see R. G. Skrynnikov, *Sibirskaiia ekspeditsiia Ermaka* (Novosibirsk, 1982); David N. Collins, "Russia's Conquest of Siberia: Evolving Russian and Soviet Historical Interpretations," *European Studies Review*, 12 (1982): 17–44.

<sup>50</sup> Another indication of the strong nationalist inspiration underlying these works was that they emphasized for the first time the importance of the common people (*narod*) to the conquest and colonization of Siberia. Collins, "Russia's Conquest of Siberia," 20–21.

<sup>51</sup> Nebol'sin, *Pokorenie Sibiri*, 138–40; Mirzoev, *Istoriografiia Sibiri*, 223. Nebol'sin's interpretation of the Siberian chronicles was the object of an uncharacteristically vituperative critique by Sergei M. Solov'ev, whose monumental history of Russia began to appear in this period. *Istoriia Rossiï s drevneishikh vremen*, 15 vols. (Moscow, 1960–66), 3: 715–23.

ized in Russian belles-lettres. An assortment of self-styled “historical novels” based on the tale of Ermak’s exploits appeared during this period,<sup>52</sup> and a number of dramatic works for the theater were composed on this theme as well. The playwrights included such notables as the historian and publicist Nikolai Polevoi and the Slavophile Aleksei Khomiakov.<sup>53</sup> The appeal of Siberian history proved infectious for no less a literary giant than Alexander Pushkin himself. The great poet was attracted by the charisma of the Cossack conquerors of Siberia, whom he saw as courageous, almost superhuman, figures embodying the glory of Russian national history. Already in the 1820s, he wrote of his desire to compose an epic poem about Ermak, and at the time of his death he was collecting material on the Russian Far East, apparently in preparation for a work about Atlasov, the conqueror of Kamchatka.<sup>54</sup>

Throughout all this literary attention, it may be noted, the Romantic “poetic formula” of Siberia persisted: the principal focus was on the heroic deeds of the Russians, while the region itself, depicted in the dimmest of colors, represented nothing more than a stage for the performance of these deeds. As in the poetry of exile, this bleak setting was intended to bring out into yet fuller relief the noble and glorious self-sacrifice of the Cossack protagonist. The alien desolation of the Siberian landscape was embellished through depictions of the native peoples encountered by Ermak as depraved pagan savages, a prospect that offered the Russians an easy opportunity to cast themselves in the appealing role of Christian crusaders bearing a mission of civilization and spiritual enlightenment. In Polevoi’s play, for example, the hero’s victory in a wrestling match with a Siberian Tatar prince occasioned the following reflective aside:

<sup>52</sup> A. A. Shishkov, *Ermak: Povest’ v stikhakh* (Moscow, 1828); P. P. Svin’in, *Ermak ili pokorenie Sibiri: Istoricheskii roman XVI veka* (St. Petersburg, 1834); I. Glukharev, *Inoki, ili vtorichnoe pokorenie Sibiri* (Moscow, 1834); F. Isaev, *Ermak, pokoritel’ Sibiri: Istoricheskii roman v 2-kh chasti* (Moscow, 1845); P. I. Neboi’sin, *Ermak: Istoricheskii rasskaz dlia detei v 2-kh chasti* (St. Petersburg, 1849). The popularity of this theme is indicated by the fact that the anonymous work *Ermak, pokoritel’ Sibiri: Istoricheskii roman v 2-kh chasti* (Moscow, 1839), had gone through five editions by 1845.

<sup>53</sup> *Ermak, zavoevatel’ Sibiri: Istoricheskaiia p’esa* (Moscow, 1827); A. S. Khomiakov, *Ermak: Tragediia v piati deistviiakh* [1832], *Polnoe sobranie sochinenii* (Moscow, 1900–14), 4: 305–418; N. A. Polevoi, *Ermak Timofeich, ili Volga i Sibir’: Dramaticheskoe predstavlenie v 5-ti deistviiakh* (St. Petersburg, 1845). The Ermak theme became so well-worn that in the mid-1840s Fedor Dostoevsky poked fun at it in his novel *Poor Folk*. See *Polnoe sobranie sochinenii*, 30 vols. (Leningrad, 1972–88), 1: 51–53 (I am grateful to Professor Victor Terras for calling my attention to this reference). For a discussion of the literary image of Ermak in various periods, see S. Rozhnova and N. Kurdina, “Ermak istoricheskii i literaturnyi,” *Sibirskie Ognii*, 12 (1981): 156–64.

<sup>54</sup> Letter of February 23, 1825 to N. I. Gnedich, in A. S. Pushkin, *Polnoe sobranie sochinenii*, 10 vols. (Leningrad, 1977–79), 10: 100; A. S. Pushkin, “Zametki pri chtenii *Opisaniia zemli Kamchatki* S. P. Krashenninnikova,” in *Polnoe sobranie sochinenii*, 9: 321–49. On the subject of Pushkin’s Siberian interests, see especially V. I. Selinov, “Proshloe Kamchatki v krugie istoricheskikh interesov Pushkina,” in G. F. Kungurov, ed., *A. S. Pushkin i Sibir’* (Irkutsk, 1937), 152–54; Georgii Vernadskii, “Pushkin kak istorik,” *Uchenye Zapiski Russkoi Uchenoi Kollegii v Prage*, 1 (1924): 63; A. A. Bogdanova, “Pushkin i Sibir’,” *Uchenye Zapiski Novosibirskogo Pedagogicheskogo Instituta* (Seriiia istoricheskoi-filologicheskoi), 7 (1948): 8; A. V. Gurevich, *Pushkin i Sibir’* (Krasnovarsk, 1952), 51–73; E. Titov, “Pushkin—Sibir’—Amerika,” *Sibirskie Ognii*, 3 (May–June 1937): 113–26. Pushkin had an opportunity to learn about Siberia from his correspondence in the mid-1830s with his old friend the Decembrist V. K. Kiukhel’beker, exiled east of Lake Baikal. Pushkin corresponded as well with one of the great experts on Siberia, Grigorii Spaskii. V. I. Saitov, ed., *Sochineniia Pushkina*, 3 vols. (St. Petersburg, 1906–11), 3: 26, 278–80, 359–62, 385–86. For a recent tendentious depiction that vaguely identifies a Great Russian chauvinism in Pushkin’s views of Siberia, see S. Iu. Kuniaev, “Velikii put’,” in A. Perlovskii, ed., *Sibirskie stroki* (Moscow, 1984), 5–6.

As the pagan idol fell, so will fall  
 The infidel before the Orthodox faith,  
 And divine grace and light will shine  
 Over the Siberian realm, which hitherto  
 Has stagnated in the darkness of idolatry.<sup>55</sup>

In its capacity as the benevolent civilizer of Asia, the nationalists could locate yet another example of Russia's noble and humanitarian national achievement.<sup>56</sup>

The urge for self-awareness had an explicitly geographical dimension as well. The nationalists bewailed the ignorance of Russians about their own country, and on the pages of nationalist journals such as Pogodin's *Muscovite*, exasperation and scorn were vented on "our Europeans," in other words, those Russians who were "enraptured by foreign [European] lands and do not appreciate the diversity of our fatherland."<sup>57</sup> As a response to this situation, a variety of descriptions and travel accounts of Siberia aimed at the general reading public began to appear in the 1830s. Readers in Moscow and St. Petersburg were treated to evocative portrayals of the region's multifarious characteristics: its physiography, climate, flora and fauna, the colorful ethnographic diversity of its native populations, and the folk traditions of the Russians there. Russian traditions, as might be expected, received particular emphasis. One author spent much of her book describing how ancient Russian folk customs and rituals, long forgotten in European parts of the country, survived in an unadulterated form in Siberia, and to illustrate this she appended a lengthy collection of old Siberian wedding songs.<sup>58</sup> In contrast to historical fiction, these works depicted Siberia in a positive light, for the intention was to make Russians aware of the exotic and unappreciated wonders within their own empire. The proliferation and popularity of such regional descriptions testified to the fact that, seen through nationalist eyes, the prospect of Siberia could now fascinate and even inspire, as well as appall.<sup>59</sup>

In an emphatic departure from the grim poetic formula, some Romantic writers discovered that in Siberia's unspoiled landscape they could regain a pure, inspirational bond with the organic natural world, a bond long lost in civilized and despiritualized European Russia. Although Siberia was never to become the sort of Olympian realm that Romantic authors in Western Europe created in the form of the "Orient"—that "sea of poetry," in Victor Hugo's expression, saturated with transcendental meaning and spiritual depth—some Russian poets during this

<sup>55</sup> Polevoi, *Ermak Timofeich*, 93.

<sup>56</sup> In the introduction to his history of the conquest of Siberia, Nebol'sin wrote: "Russians for some reason grow uncomfortable when . . . they hear the new epithet 'the civilizer' given to England. They [should] know that our Russia, no less than England, can have pretensions to the title 'enlightener,' that we also had our Cortes and our Pizarros"; *Pokorenie Sibiri*, 2.

<sup>57</sup> M. Zenzinov, "Pis'mo k redaktoru iz Nerchinska," *Moskvitianin*, part 3, no. 11 (1844): 234–35.

<sup>58</sup> E. A. Avdeeva, *Zapiski i zamechaniia o Sibiri* (Moscow, 1837), 67, 95–142. Also see A. I. Matros, *Pis'mo o vostochnoi Sibiri* (Moscow, 1827); I. S. Pestov, *Zapiski ob Eniseiskoi gubernii Vostochnoi Sibiri* (Moscow, 1833); N. S. Shchukin, *Poezdka v Iakutsk* (St. Petersburg, 1833); F. I. Beliaevskii, *Poezdka k Ledovitomu moriu* (Moscow, 1833); P. A. Slovtsov, *Progulki vokrug Tobol'ska v 1830 g.* (Moscow, 1834); Gedenshtrom, *Otryvki o Sibiri*; Parshin, *Poezdka v zabaikal'skii krai*.

<sup>59</sup> See the unsigned review of Matros, *Pis'mo o vostochnoi Sibiri*, in *Syn Otechestva*, 114 (1827): 193–96.

period nevertheless found it possible to find inspiration and regeneration through sojourns east of the Urals.<sup>60</sup>

Still more material for positive images became available at this time with the emergence of a regional literature, as Siberian writers such as Polevoi, Ivan Kalashnikov, Nikolai Shchukin, and others began to use themes and settings from their homeland as a basis for their stories. Here again, the Russians derived their inspiration for a new image of Siberia from an American model: the genre of American frontier literature developed throughout the 1820s and 1830s in the works of James Fenimore Cooper. Cooper's novels were all quickly translated into Russian, and they enjoyed there a remarkably widespread and enduring popularity. Among other things, their success suggested that the theme of a native Russian frontier east of the Urals might be developed in the same way, and the writers just noted set about to do precisely that.<sup>61</sup> The similar treatments of the two frontiers were not lost on the more astute readers in European Russia: critic Vissarion Belinsky, for example, who was quite taken with Cooper's depictions of the American original, was less than impressed by his Siberian followers and referred to them with ill-concealed condescension as "our Siberian Coopers."<sup>62</sup> Their works extolled the wild and elemental beauty of the Siberian landscape and the frontier character of its society, engaged in a life-or-death struggle to tame and cultivate primeval expanses. The result was an image that in many respects did resemble the North America of Cooper's tales. The exotica of Siberia was depicted positively, so much so that, in some cases, the region appeared as a sort of natural fairyland. Nikolai Shchukin, for example, began his story *The Settler* (1834) with the following extravagant description of eastern Siberia:

How vast and varied is your world of nature, Siberia! And how contrasting the peoples and tribes that inhabit you! Your granite mountains, reaching up to the clouds, are splendid under their snowy blankets. And meadows, fragrant with the aroma of flowers, entice the gaze of the traveler with their diversity. Cliffs and chasms, silhouetted picturesquely against mountains covered with dense forests, reveal flanks overgrown with blossoming bushes: from lofty heights the mountain slopes gleam golden with thick ears of ripening grain. Here the mightiest rivers in the world, coursing rapidly into the depths of the ocean, carry on their crests the fruits of your industry. Streams as clear as crystal flow from the

<sup>60</sup> "Là, en effet, tout est grand, riche, fécond"; Victor Hugo, *Les Orientales* [1829] (Paris, n.d.), 4; Schwab, *Oriental Renaissance*, 12–13. See, on Russian poets, E. P. Kovalevskii, *Sibir': Dumy* (St. Petersburg, 1832). For discussions of Romanticism and the Russian east, see M. K. Azadovskii, "Buriatiia v russkoi literature," *Zhizn' Buriatii*, 1–2 (January–February 1925): 10–19; Azadovskii, "Poetika 'giblogo mesta'"; Slezkine, "Russia's Small Peoples," 147–50. Even more than Siberia, however, the Caucasus to the south served as an ersatz Orient for Russian Romantic writers. See Layton, "Creation of an Imaginative Caucasian Geography"; Susan Layton, "Marlinsky's 'Ammalat Bek' and the Orientalisation of the Caucasus in Russian Literature," in Derek Offord, ed., *The Golden Age of Russian Literature and Thought* (forthcoming); William E. Brown, *A History of Russian Literature of the Romantic Period*, 4 vols. (Ann Arbor, Mich., 1986), 2: 87.

<sup>61</sup> For a discussion of this formative period of Siberian literature, see Postnov, *Russkaia literatura Sibiri*, 173–314; Gurevich, *Vostochnaia Sibir' v rannei khudozhestvennoi proze*, iii–xxiii; M. K. Azadovskii, "Sibirskaiia belletristika tridsat'kh godov," in Azadovskii, *Ocherki literatury i kul'tury Sibiri*, 5–105; B. I. Zhrebtsov, "O sibirskoi literaturnoi traditsii: Nabliudeniia i zametki," *Sibirskii literaturno-kraevedcheskii sbornik*, 1 (1928): 23–50.

<sup>62</sup> V. G. Belinskii, "Literaturnye mechtaniia" (1834), *Sobranie sochinenii v trekh tomakh* (Moscow, 1948), 1: 10; G. F. Kungurov, "V. G. Belinskii o tvorchestve pisatelei-sibiriakov," *Uchenye Zapiski Irkutskogo Gosudarstvennogo Pedagogicheskogo Instituta*, 12 (1957): 21–24.



heights of the clouds, and falling from rock to rock disappear into bottomless gorges. Bursting forth anew from deep underground, they rush into Lake Baikal, pulling heavy stones in their wake. Dig deep into the mountains and you will find silver, gold, and glittering jewels. And then won't you agree: Siberia is a rich land!<sup>63</sup>

Through this rather mediocre prose, the author seems indeed to have taken his cue from Cooper, who played heavily on the natural wonders of the American West, the exotic and inspirational grandeur of its titan forests and the vast sweep of the Great Plains.<sup>64</sup>

A REVEALING VARIATION on the images of Siberia generated by Russian nationalism was developed by Alexander Herzen, one of the most brilliant and engaging intellectuals of the period. Herzen's perspective, which combined an emergent nationalism with the older tradition of political opposition to the Russian autocracy, presented a unique example of the important function Siberia could fulfill in the development of a new, radically de-Europeanized vision of Russia. Herzen's first contact with the Russian east dated from 1835, when as a young man in his early twenties he was sent in so-called administrative exile to Viatka (present-day Kirov) as punishment for his activities in a student group critical of the regime. He spent a total of five years in exile, in Viatka and later in Vladimir. At the time of his banishment, Herzen's political opposition to the status quo in Russia was more or less clearly formulated, but his nationalism per se had not yet taken shape. The West still represented an admirable model for him; he still had faith in Peter's mission, indeed, he believed that the desired political reform of Russian society would come about precisely through its complete merging into the European mainstream.<sup>65</sup> He looked on his impending exile as a fate not much better than death, and he embarked on it with a dread similar to that of the Decembrists. In his autobiography, he described how en route to Viatka he was overcome by a sense of gloomy foreboding and was moved at a way station to jot down the verses from Dante's *Inferno*

Through me you enter the woeful city,  
Through me you enter eternal grief.

as being "equally well adapted for the road to Siberia as to the gates of Hell."<sup>66</sup>

Herzen's reference to the "road to Siberia" was in fact gratuitous, for the town of Viatka, in the upper basin of the Kama River, was situated due north of Kazan and consequently several hundred miles west of the Urals. Herzen never actually saw Siberia, and his information about it came entirely from hearsay and second-hand accounts. His physical location proved to be irrelevant, however, for

<sup>63</sup> Quoted in Kungurov, "Belinskii o tvorchestve," 19–20. For a similar depiction of the Siberian landscape, see Polevoi's story "Sokhatyi (Sibirskoe predanie)," in *Dennitsa: Al'manakh* (Moscow, 1830), 272–[349].

<sup>64</sup> Billington, *Land of Savagery, Land of Promise*, 31.

<sup>65</sup> Malia, *Herzen*, 96–97, 125–27.

<sup>66</sup> A. I. Gertsen, *Byloe i dumy*, in *Sobranie sochinenii v 30-kh tomakh* (Moscow, 1954–65), 8: 219; the verse is from *The Inferno*, Part 1, Canto 3.

Aus den

## Memoiren eines Russen.

Im Staatsgefängniß

und

in Sibirien

von

Alexander Herzen,

Verfasser des „Dem anderen Hier“, der „Briefe aus Italien und Frankreich“ und „Aufstände ferne Zukunft“.

Hamburg.

G e r m a n n u n d C o m p e.  
1855.

Alexander Herzen at the time of his exile to Viatka, and the title page to the first German edition of Herzen's account of his "Siberian" exile. A. I. Gertsen, *Sobranie Sochinenii v tridtsati tomakh* (Moscow, 1954), vol. 1, p. 273. The portrait is by A. L. Vitberg and hangs in the State Tretyakov Gallery, Moscow.

his psychological need to envision an alternative to official autocratic Russia in the form of a geographical Other was as strong in him as in the Decembrists. Predictably, his negative pre-exile images of Russia's eastern provinces evaporated in an identical fashion almost immediately on arrival at Viatka, and he proceeded to develop an articulated vision of Siberia with an air of authority suggesting that he had spent long periods there and knew it intimately. Indeed, on the level of the perceptions and images that concern us in this essay, his interaction with the region was quite as intense and significant as if he did.

Herzen extolled the qualities of freedom and egalitarianism he perceived in the Russian society in Siberia and in his exuberance far outdid the Decembrists. He was impressed by the absence of a landed nobility and urban aristocracy; he marveled at the outspoken sense of independence on the part of the Siberians, a characteristic rarely encountered in European Russia. He described with great satisfaction the general disdain for official representatives of tsarist authority, the *chinovnik*, or government bureaucrat, and the *gendarme*, who in Siberia were appropriately regarded by the local population "more like an occupying foreign garrison installed by a conquering power" than the respectable and legitimate upholders of the social order. The Siberian peasant, unburdened by the oppressive weight of serfdom, compared in his estimation entirely favorably to his Great Russian counterpart, in terms of both physical well-being and intelligence. The great distances in Siberia and the associated isolation of rural settlements made the Siberian much more self-reliant, resourceful, and—of no little importance—ready when necessary to manifest resistance. Herzen noted the relatively infrequent contact of the average Siberian with the church, owing again to the great distances and isolation, and saw this as decidedly beneficial, for it has "left his mind freer from superstition" than his counterpart's in European Russia.<sup>67</sup>

Underlying these positive manifestations of Siberian society, Herzen perceived a deeper principle at work. On reflection, it seemed obvious to him that Siberia's very backwardness, its lack of advanced development and refinement, had conditioned the qualities that so impressed him. To be sure, the backwardness he had in mind did not refer to a complete absence of civilization, for Herzen did not follow Rousseau in a categorical rejection of modern enlightened and developed society per se, nor did he share (as, indeed, few Russians did) the Romantic conviction about the inherent superiority of man in his natural state. It was, after all, not Siberia's indigenous native peoples—who in principle could have offered abundant material for a Russian variation of the "noble savage" theme—that he found appealing but rather its Russian population, and he valued the rudimentary institutions of Western society they had established. Nevertheless, when compared to the cosmopolitan enlightenment of Europe, including European Russia, Siberia remained undeniably primordial, with its absence of accumulated historical traditions and patterns, its elemental rawness and outspoken lack of cultivation. While it was precisely these qualities that in the past had occasioned such abhorrence, they proved to be powerfully attractive to Herzen. He described

<sup>67</sup> Gertsen, *Sobranie sochinenii*, 8: 256–57.

his reactions in an enthusiastic letter written to friends back in Moscow shortly after his arrival in 1835:

What is Siberia?—here is a country that you do not know at all. I breathed in the icy air of the Urals: it was cold *but fresh and healthy*. Do you know that Siberia is an entirely new country, an America *sui generis*, precisely for the reason that it is a land without aristocratic origins, the daughter of the Cossack and brigand, which doesn't remember its forebears, a country in which people are renewed, closing their eyes on their entire past . . . Here everyone is an exile and everyone is equal . . . Back there [in European Russia] life is enjoyable and enlightened, but the most important things are freshness and newness.<sup>68</sup>

Siberia was a society *in statu nascendi*, unspoiled as yet by an oppressive autocratic tradition, which, in exchange for the urbane sophistication of Europe, possessed all the energy, dynamism, and plasticity of youth.

The fundamental contradiction between this newly discovered virtue of historical youth and Herzen's older faith in the West European tradition was resolved in the years following his return from exile in 1840. He became increasingly skeptical of the assumption that the progressive reform of the Russian social and political order was possible only through its far-reaching Europeanization. Along with the other nationalists, Herzen turned his attention inward and began to scrutinize Russia itself for native elements that would make possible a different, specifically Russian, course for future development. His most famous discovery in this regard—suggested at least in part, rather ironically, by the studies of a conservative German agricultural economist, Baron August von Haxthausen<sup>69</sup>—was the rural social institution of the peasant commune (*obshchina*), unique to Russia, which Herzen imagined could serve as the natural basis for a future socialist society and make it unnecessary for Russia to repeat Europe's long and painful period of capitalist development. Out of this same quest, Herzen became increasingly preoccupied with the positive qualities he had noted in the Russian east, and, as his perspective evolved, these qualities were gradually dislodged from their specific geographical context. More and more, it seemed to him that the characteristics of youthful freshness and lack of oppressive tradition, which in the 1830s had appeared unique to Siberia and in favorable contrast to traditional society in Russia west of the Urals, actually fit Russia as a whole: not, to be sure, official imperial Russia represented by the autocratic despotism of Nicholas I but rather the genuine Russia of the unspoiled common people (*narod*). From this standpoint, Russia's youth, its inexperience, and even its lack of historical contribution to world civilization could all be seen as factors working in its favor, for they would allow Russia to avoid repeating Europe's tortured and ultimately stymied process of development. The latecomer has the advantage of being able to reap the benefits of everyone else's experience and mistakes, he reasoned, and

<sup>68</sup> Letter of July 18, 1835 to N. I. Sazonov and N. Kh. Ketcher in *Sobranie sochinenii*, 21: 45–46. For a fuller discussion of this passage, and Herzen's exile in general, see Bassin, "Russian Mississippi?" 54–55.

<sup>69</sup> August von Haxthausen, *Studien über die innern Zustände, das Volksleben, und insbesondere die ländlichen Einrichtungen Russlands*, 3 vols. (Hanover, 1847–52). Herzen had met and had a lengthy exchange of views with von Haxthausen in 1843, during the latter's sojourn in Russia. S. Frederick Starr, "Introduction," A. von Haxthausen, *Studies on the Interior of Russia*, E. L. M. Schmidt, trans. (Chicago, 1972), xx, xxxiii; Malia, *Herzen*, 310–11.

concluded defiantly that “progress itself is nothing but this chronological ingratitude.”<sup>70</sup>

With this, the original contrast between Siberia and Russia was transformed into a juxtaposition between Europe or the Old World on the one hand and all of Russia on the other. What had been Siberia’s historical youth, and associated dynamism and potential, now became Russia’s and entered the nationalists’ arsenal of recently discovered native virtues, where it helped illuminate a vision of Russian national exclusivity from and superiority over Europe.<sup>71</sup> As a telling indication of this transformation, the older view of Siberia as an “America *sui generis*” was subtly but unmistakably recast, with Russia itself substituted for Siberia.

Herzen was still very far from this notion in the 1830s, at which time he reaffirmed Siberia’s affinities with the United States and pointedly rejected any suggestion of a comparison between America and Russia.<sup>72</sup> Signs of a change were apparent in his thinking already in the mid-1840s,<sup>73</sup> but it was fully articulated only after he departed Russia in 1847 to take up permanent residence in the West. His disappointment with the materialism and injustice of European society, his deep despair over the failed revolutions of 1848, and finally the eruption of collective European belligerence against Russia in the form of the Crimean War all convinced him that the Old World had lost its potential for creative action. The principal arenas for the development of civilization in the future would have to be found elsewhere, and the alternative was ready at hand. He could point confidently to the elemental affinities between the United States and Russia as youthful, vigorous societies. Throughout the 1850s, he proceeded to expound a vision of an imminent transition in world civilization, a sort of historical succession, in the process of which the dynamism and energy of these non-European realms would triumph over and replace the “dreary stagnation” that dominated in Europe.<sup>74</sup> Here, then, was the identical juxtaposition between the Old and New Worlds—and all the attendant implications—that had been drawn by the Decembrists, with the one fundamental difference that European Russia had been shifted out of the Old World and into the New.

The vision of an elemental Russian-American affinity reached a high point in the late 1850s. It was stimulated by developments in the far southeastern corner of Siberia itself, where the Russians were busy with the occupation and annex-

<sup>70</sup> “Discours d’Alexandre Herzen . . .” (1855), *Sobranie sochinenii*, 12: 251.

<sup>71</sup> Malia, *Herzen*, 143–44.

<sup>72</sup> Writing to a friend in 1838, for example, Herzen described the dismay that Tocqueville’s comparison between the two countries caused him. Letter of August 20 to N. Kh. Ketcher, *Sobranie sochinenii*, 21: 386. Also see the play he wrote in 1839, based on the experience of William Penn’s Quaker settlement, in which North America was depicted as a remote foreign country, without any suggestion of affinities with Russia; “Vil’iam Pen,” in *Sobranie sochinenii*, 1: 196–250; David Hecht, *Russian Radicals Look to America 1825–1894* (Cambridge, Mass., 1947), 23; Alexander Kucherov, “Alexander Herzen’s Parallel between the United States and Russia,” in J. S. Curtiss, ed., *Essays in Russian and Soviet History in Honour of Geroid Tanquary Robinson* (Leiden, 1963), 34.

<sup>73</sup> Malia, *Herzen*, 308.

<sup>74</sup> Alexander Herzen, “Pis’mo k Dzhuzeppe Matstini o sovremennom polozhenii Rossii” (1857), *Sobranie sochinenii* 12: 349; “Staryi mir i Rossiia” (1854), in *Sobranie sochinenii*, 12: 169; Laserson, *American Impact on Russia*, 206–11; Kucherov, “Alexander Herzen’s Parallel,” 36–37; Edward Acton, *Alexander Herzen and the Role of the Intellectual Revolutionary* (Cambridge, 1979), 82.



ation of the Amur and Ussuri river regions from the Chinese empire. Herzen, along with most of the Russian public in the period immediately following Russia's defeat in the Crimean War, was thrilled by the prospect of these territorial advances. Not only did they demonstrate the country's unbroken spirit and continuing capacity for resolute self-assertion but they did so in a manner that turned Russia dramatically away from Europe to face its sister nation across the Pacific, a development happily greeted by the Americans. Herzen celebrated the conclusion of the Aigun Treaty in 1858, which formalized these territorial acquisitions, with a lead article in his newspaper *Kolokol*, and this brief essay stands as a culmination to the process I have been describing. The debacle of the Crimean War, he wrote, left no question about the pointlessness of Russia's attempts to enter the European community. However, it provided Russia with the opportunity to take its "moral liberation" from the West. The events of the day were revealing Russia as a youthful organism, "a strong and capable embryo," in Herzen's phrase, which had as its primary task to look inward and discover "elements of the future in the crude principles of its own life." In this struggle to reestablish its independence and national dignity, Russia had only one true ally. Deep affinities overcame all differences between their societies and drew them together to share a common destiny:

Both countries abound in strengths, an ability to change, a spirit of organization, and a persistence that knows no obstacles. Both are poor in tradition and take as their first step a complete break with the past; both swim through endless valleys searching for their borders, and from different sides have traversed awesome expanses. They have everywhere marked their path with cities, villages, and colonies, up to the shores of the Pacific Ocean, the 'Mediterranean of the future' . . . At the time of Europe's gloomy sepulture, when everyone has something to bewail, [the Russians] from one side—and the Americans from the other—have put the cradle back together!<sup>75</sup>

To the extent that Herzen asserted Russia's youth, its plasticity, freshness, potential for progressive future development, and independence from the European Old World—to this extent, it may be said that his nationalist vision had been Siberianized. Indeed, he unwittingly drew attention to this very point, for, although he titled his article "Siberia and America" and evoked thereby the original comparison of the 1830s, he did not mention Siberia once in the text; all of his points were concerned exclusively with Russia. Herzen eventually noted this discrepancy, and, when he reprinted the article verbatim ten years later, it bore the more accurate title "America and Russia."<sup>76</sup>

The parallel Herzen drew in the above passage between the transcontinental colonizing movements of the two countries suggested a new dimension to the

<sup>75</sup> Iskander [Alexander Herzen], "Amerika i Sibir'," *Kolokol*, 29 (December 1, 1858): 134. This piece was a response to an article that appeared in the *Philadelphia Evening Bulletin*, congratulating the Russians on their new ensconcement on the Pacific. The Americans were so enthusiastic that the publishers of the *Evening Bulletin* actually had a leaflet, printed in English, inserted into the edition of *Kolokol*, encouraging the Russians to pursue the development of the Amur region with all due haste. Laserson, *American Impact on Russia*, 216–18. Also, see Svatikov, *Rossia i Sibir'*, 40–42. For a study of the views of Russia's educated public on expansion in the Far East at this time, see Mark Bassin, *Visions of the Amur: Nationalism, Imperial Expansion, and the Emergence of Russia as a Modern Pacific Power* (forthcoming).

<sup>76</sup> "Amerika i Rossiia," *Kolokol*, 228 (October 1, 1866): 1861–62.

vision of Russia as a “civilizer” beyond the Urals. Rather than simply affirm an arrogant self-satisfaction with the conquest and spiritual redemption of an ignorant heathen population, Herzen emphasized the creative accomplishments of the Russians in response to the challenge of mastering an inhospitable natural environment. For this type of endeavor, the United States offered the essential contemporary prototype, and in his treatment of this point Herzen repeated the conceptual process of assigning a significance to Siberia appropriated from the North American model. Just like the Americans in their restless expansion across the North American continent, the Russians had worked to advance civilization through their activities in “taming” and making productive the primeval expanses of Eurasia.<sup>77</sup> Herzen saw this process exemplified in the Russian occupation of the Amur Valley, but he felt that it had been characteristic of the Russian presence beyond the Urals throughout the centuries. The parallel with the United States seemed so appropriate that he even used the American frontier experience as a metaphor to describe Russia’s own expansion across Siberia. In a letter to Giuseppe Mazzini, he likened the first colonists of the Urals and western Siberia to the settlers in North America homesteading “on the virgin lands of Wisconsin or Illinois” and maintained that the Siberian experience “was right out of one of Fenimore Cooper’s novels.” And, just like the Americans, the Russians had accomplished their own miracle, for they had brought empty territories into the pale of modern civilization by erecting cities, hospitals, and schools and introducing modern commercial activity.<sup>78</sup> Depicted from this perspective, the occupation and development of Siberia served admirably as evidence of the Russian capacity for independent historical achievement.

Yet, at this point, in regard to the apparent parallel in their respective colonizing movements, Herzen’s comparison between Russia and America foundered on a subtle but nonetheless fundamental ambiguity. While on one level, he was most eager to interpret Russian expansion to the Pacific positively, as the same sort of healthy and dynamic organic growth that characterized the United States, on a deeper level, the same process was imbued with a thoroughly negative significance, for it was an expression of the incorporation of foreign peoples and regions under the despotic aegis of the Russian imperial autocracy. Herzen tried to accommodate this ambiguity with the rather obscure identification of a “dualism” between popular and governmental colonization,<sup>79</sup> but it was the negative perspective that dominated, for an intractable opposition to the imperial system symbolized by St. Petersburg represented one of his most abiding political principles. For Herzen, the empire was something entirely foreign to Russia, a “Germanic-Tatar despotism” as he liked to call it, comprised of a misshapen and perverse blend of European absolutism and Oriental tyranny, all artificially

<sup>77</sup> Kucherov, “Alexander Herzen’s Parallel,” 36.

<sup>78</sup> “Pis’mo k Dzhuzeppe Matstini,” 350.

<sup>79</sup> “I-r” [Alexander Herzen], “Rossiia i Pol’sha,” *Kolokol*, 67 (April 1, 1860): 556; also see “Pis’mo k Matstini,” 349–50. This ambiguity was epitomized by the fact that Herzen could compare Russian expansion not only with the United States but with the British empire as well; see Iskander, “Rossiia i Pol’sha,” *Kolokol*, 32–33 (January 1, 1859): 259.

grafted onto Russia by Peter the Great.<sup>80</sup> In unmistakable contrast to the free and healthy expansion of the United States, the Russian empire had grown by coercion and military decree, a process Herzen described disagreeably as an “oozing” (*prosochit’sia*) out of the original Russian hearth to the Baltic, to Persia, and to the Pacific. The imperial formation that resulted was destructive not only to the interests of the Russian nation itself but to all of the peoples and lands banded together, entirely against their natural inclinations and best interests, as part of it.<sup>81</sup>

Herzen believed that the destruction of official Russia would unquestionably involve the loosening of this band to allow the non-Russian borderlands to pursue an independent existence as they saw fit. In the first instance, he made this point in support of the liberation of the empire’s western provinces of Poland, Lithuania, Belorussia, and the Ukraine, but at the same time clearly included Russia’s territories east of the Urals as well.<sup>82</sup> The notion of the “liberation” and independence of Siberia was not entirely new, but Herzen was the first to give it public expression.<sup>83</sup> He asserted in effect that the right of self-determination belonged not only to national groups but to territorial units as well, if they were internally cohesive and distinct from the Great Russian center. Siberia in his eyes represented such a unit. In all probability, he initially received this idea from Siberia itself, where the flurry of excitement and activity accompanying the annexation of the Amur had stimulated ideas about the potential for Siberia’s development as an independent political entity. In the early 1860s, Herzen had a number of correspondents in Irkutsk—among them, the anarchist Mikhail Bakunin—who advocated the notion of Siberia’s formal separation from Russia,<sup>84</sup> and Herzen himself eagerly endorsed the notion. In 1862, for example, he editorialized: “We recognize the right to an independent existence [*samobytnost’*] not only of every nationality, having separated from others and having natural borders, but of every geographical region [*geograficheskoe polozhenie*] as well. If Siberia tomorrow were to separate from Russia, we would be the first to greet its

<sup>80</sup> “Staryi mir i Rossiia,” 172; Iskander, “Rossiia i Pol’sha,” *Kolokol*, 65–66 (March 15, 1860): 539, 543; *ibid.*, “Pis’mo k Matstsinu,” 352.

<sup>81</sup> Many decades later, in the 1890s, when a massive new wave of peasant migration from European Russia east to Siberia had begun, and work was finally undertaken on the construction of a transcontinental railway across Siberia to the Pacific, a parallel between the colonizing movements of Russia and the United States was again drawn, this time more directly and without any of Herzen’s ambivalence. See P. P. Semenov, “Znachenie Rossii v kolonizatsionnom dvizhenii evropeiskikh narodov,” *Izvestiia Imperatorskogo Russkogo Geograficheskogo Obshchestva*, 28 (1892): 349–69; Vernadskii, “Protiv solntsa.”

<sup>82</sup> “I-r,” “Proklamatsiia ‘zemli i voli,’” *Kolokol*, 160 (April 1, 1863): 1318; “Ukraina,” *Kolokol*, 61 (January 15, 1860): 499–503; Iskander, “Rossiia i Pol’sha,” *Kolokol*, 65–66 (March 15, 1860): 539, 541.

<sup>83</sup> This prospect had, for example, been discussed by the Petrashevskii circle in the 1840s. V. A. Desnitskii, ed., *Delo Petrashevtssev*, 3 vols. (Moscow-Leningrad, 1937–51), 1: 462; P. E. Shchegolev, ed., *Petrashetsy: Sbornik materialov*, 3 vols. (Moscow-Leningrad, 1926–28), 2: 252–53; 3: 18, 244.

<sup>84</sup> *Pis’mo M. A. Bakunina k A. I. Gertsenu i N. P. Ogarevu* (St. Petersburg, 1906 [reissued as Vol. 111 of *Slavistic Printings and Reprintings* (The Hague, 1968)]), 123; V. Modestov, “Zagranichnye vospominaniia,” *Istoricheskii Vestnik*, 12 (April 1883): 122; “Iz Sibiri,” *Kolokol*, 131 (May 1, 1862): 1092. The young Peter Kropotkin, serving in a Cossack regiment in eastern Siberia at this time, reported that even Governor-General N. N. Murav’ev was attracted by the notion of an independent “United States of Siberia”; *Memoirs of a Revolutionist* (New York, 1971), 169, 184.

new life. The territorial unity of the state does not coincide at all with the well-being of the peoples [who inhabit it].”<sup>85</sup>

RUSSIAN VIEWS OF SIBERIA were examples of the same sort of “imaginative geography” that resulted from European attempts to depict and understand the non-European world as a whole. In Russia, as in the empires to the west, the character and significance of colonial domains were represented in terms of categories and attributes meaningful in the first instance to those doing the representing, and thus the considerable differences in the images of the Russian east that were constructed in the early nineteenth century reflected more than anything the growing political and cultural fragmentation of European Russian society in this period. For the autocratic status quo, Siberia was nothing more than a withered and useless remnant of past colonial glory, and the prospect of its wild and primeval environment and inhabitants terrorized the civilized sensibilities of those Russians west of the Urals still consciously striving to become as European as possible. By contrast, reformers and political opponents of this status quo could see Siberia very differently, as the home of a democratic and egalitarian society, a positive alternative to Russia west of the Urals in which the notion of the rights of man had a meaning and was respected. In a real sense, they believed that Siberia afforded a precious glimpse into the sort of future they desired for Russia as a whole. Russian nationalists, seeking militantly to countervail the conscious self-conscription of their compatriots into the sphere of European civilization, constructed their own Siberia: both an illustrious historical chronicle that bore witness to the grand exploits and accomplishments of intrepid Russian explorers and settlers, and a geographical wonderland, the unique richness and diversity of which added brilliance and luster to the image of the fatherland as a whole. Finally, in the thinking of Alexander Herzen, Siberia became a prototype for a new nationalist vision of Russia as a radically de-Europeanized New World, and its qualities were subtly generalized to characterize the entire country.

A number of the images of Siberia discussed in this essay have displayed a remarkable endurance and may easily be identified in contemporary attitudes in the Soviet Union. The study of these contemporary images would offer a rich and rewarding theme for further research. Despite official disclaimers, the traditional mercantile-colonial attitude unmistakably persists, and Siberia continues to be regarded as a repository of valuable resources—today no longer furs but rather petroleum, natural gas, coal, metallic and mineral ores—available for economic exploitation by other parts of the country. The experience of the economic development of Siberia over the past two decades lends credibility to Leslie Dienes’s suggestion that the function of Siberia as an “energy colony” remains

<sup>85</sup> [Alexander Herzen] “Russkim ofitseram v Pol’she,” *Kolokol*, 147 (October 15, 1862): 1214. It is interesting to note that despite the considerable attention Soviet scholars have given to Herzen’s views on Siberia, his support of Siberian separatism has up to now remained unmentioned. V. G. Kubalov went as far as to deny that Herzen ever entertained the idea that Siberia could be an independent entity. A. I. Gertsen *i obshchestvennost’ Sibiri* (Irkutsk, 1958), 58–59; also see Mirzoev, *Istoriografiia Sibiri*, 214–18.

uppermost in the minds of the planners in Moscow.<sup>86</sup> Along with this, the vision of Siberia as Russia's undeveloped frontier is kept alive, and perceptions of this frontier remain characterized by the same polarity between positive and negative discernable in the nineteenth century. The poet Evgenii Evtushenko, himself a Siberian, frequently used the motif of his homeland in his early writing and evoked a thoroughly positive sense of the region's primeval strength and force, the toughness and reckless, irrepressible self-confidence of the Siberians themselves.<sup>87</sup> By contrast, for the European Russian imagination, Siberia can still conjure up strongly negative associations as a sort of barbaric Wild West: an exotic and untamed environment far removed from "civilization," where the animals are all enormous, savage, and fearsome, the inhabitants still worship pagan idols, and the "law of the taiga" reigns supreme.<sup>88</sup>

Even the vision of Siberia as a New World and a guarantor of Russia's future prosperity survives. This interest comes today from a very different source than in the nineteenth century, for, while Siberia formerly was appealing by virtue of its democratic qualities and its affinities with a progressive model from the West, at present this image is fostered as part of a manifestly conservative, even reactionary perspective. Nevertheless, on a deeper level, the parallels are apparent. In both cases, this view is the product of articulated political dissent, which rejects the despotic corruption—in the form of tsarist autocracy or Soviet communism—of the center and believes that it has located a promising alternative in Russia's remote geographical periphery. Both focus on Siberia's freshness and lack of development, spared from despoliation and left with abundant resources and unbounded territorial expanses for settlement and constructive exploitation. This circumstance then ensures what is the most important point, the critical potential of Siberia for healthy growth and development in the future, a potential that in an important sense can offer Russia a chance to start anew. This view of Siberia was developed with some intensity during the mid-1970s in essays by Alexander Solzhenitsyn. He called for Russia to end its preoccupation with the West and its Third World entanglements and to concentrate exclusively on developing its own internal resources, most of all those of unspoiled Siberia, which comprises the bulk of what he refers to as the Russian Northeast. These regions, he asserted, are "our hope and our reservoir,"<sup>89</sup> and explained his point in the following terms:

<sup>86</sup> Leslie Dienes, "Economic and Strategic Position of the Soviet Far East," *Soviet Economy*, 1 (1985): 150.

<sup>87</sup> See especially Evtushenko's poems "I am of the Siberian breed" (*Ia sibirskoi porody*), "The Winter Station" (*Stantsiia zima*), or "Where do I come from?" (*Otkuda rodom ia?*), Evgenii Evtushenko, *Izbrannnye proizvedeniia*, 2 vols. (Moscow, 1975), 1: 40, 93–120, 154–55.

<sup>88</sup> Some examples are Andrei Skalon, "Mishkin sneg," *Sibirskii rasskaz* (Novosibirsk, 1975), 355–70; Nikolai Kuzakov, *Liubov' shamanki* (Moscow, 1975). Reacting very much as did Nikolai Iadrintsev some 125 years ago (see n. 8 above), the Siberian writer Anatolii Chernousov has denounced this contemporary penchant for shallow Siberian exotica. "Vcherashniaia ekzotika," *Literaturnoe obozrenie*, 12 (1975): 6–7. Also see V. G. Rasputin, "Sibir' bez romantiki," *Chto v slove, chto za slovom?* (Irkutsk, 1987), 27–65.

<sup>89</sup> Alexander Solzhenitsyn, *Letter to the Soviet Leaders*, H. Sternberg, trans. (New York, 1974), 29 (quote), 27, 31–32.



Fortunately, we have . . . a home, a spacious and unsullied home preserved for us by history—the Russian Northeast . . . The Northeast is a reminder that Russia is the Northeast of the Planet, that our ocean is the Arctic, not the Indian Ocean, that we are not the Mediterranean nor Africa and that we have no business there! These boundless expanses, senselessly left stagnant and icily barren for four centuries, await our hands, our sacrifices, our zeal and our love . . . [W]e should be directing our forces and urging our young people toward the Northeast—that is the far-sighted solution. Its great expanses offer us a way out of the worldwide technological crisis. They offer us plenty of room in which to correct all our idiocies in building towns, industrial enterprises, power stations and roads. Its cold and in places permanently frozen soil is still not ready for cultivation, it will require enormous inputs of energy—but the energy lies hidden in the depths of the Northeast itself, since we have not yet had time to squander it . . . Only a free people with a free understanding of our national mission can resurrect these great spaces, awaken them, heal them, beautify them with feats of engineering.<sup>90</sup>

<sup>90</sup> Alexander Solzhenitsyn, "Repentance and Self-Limitation," *From under the Rubble*, A. M. Brock, *et al.*, trans. (Boston, 1974), 140–42.

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## The Banking Crisis, Peasant Reform, and Economic Development in Russia, 1857–1861

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STEVEN L. HOCH

THE MID-NINETEENTH CENTURY IN RUSSIA was a period of frenzied social engineering in which the autocracy sought to redesign society. Serfdom was abolished, the army overhauled, government finances and the banking system reformed, provincial and local administration revitalized, the judicial system restructured, censorship eased, the universities reorganized, and railroad development begun. Few historians would dispute the fact that the aptly named “Era of Great Reforms,” the decade or so that followed Russia’s humiliating defeat in the Crimean War (1854–1856), was the most important in the history of the empire between its establishment early in the eighteenth century and its demise early in the twentieth.

For rural Russia, reform meant a diminished role for the nobility as the managers of peasant life. The emancipated serfs were to become landowners by means of a vast credit, or “redemption,” operation.<sup>1</sup> For economic development, reform signified a massive and sudden shift of capital out of monopolistic state credit institutions, which for years had provided long-term credit to serfowners and the state treasury, and into private joint-stock companies to finance railroads and steam navigation.

Researchers of the Great Reforms have been preoccupied with the motivation for emancipating the serfs and the subsequent impact of the legislation. But this has not helped explain why the legislation took the form it did. Notions that the causes of the emancipation of 1861 lay in humanitarian concerns, mounting peasant unrest, or an urgent desire on the part of the state to foster industrial development have been criticized as adding up “to nothing more than a list of motives which might reasonably appear to have influenced those making the decisions.”<sup>2</sup> Seeking the origins of the emancipation in an agrarian crisis of the feudal-serf system—a view motivated more by ideology than history—has been attempted so often by Soviet scholars that even some Western historians now pay

<sup>1</sup> Where the land-equalizing commune existed, peasant lands were held in common rather than by individuals. In those areas of the Ukraine, Belorussia, and the Lithuanian provinces where the repartitional land commune did not exist, peasant households held their land individually.

<sup>2</sup> Alfred J. Rieber, *The Politics of Autocracy* (Paris, 1966), 17. Nor has Rieber’s own attempt to “identify the issue,” serfdom as an obstacle to military reform, found wide acceptance. See Terence Emmons, “The Peasant and the Emancipation,” in *The Peasant in Nineteenth-Century Russia*, Wayne S. Vucinich, ed. (Stanford, Calif., 1968), 44–45.

it deference, though with notes of caution.<sup>3</sup> Yet we have been duly warned of “the problem of establishing causal relationships between broad socioeconomic developments and specific historical events (such as revolutions) or deliberate human actions (such as reforms).”<sup>4</sup>

More recently, some answers have been sought in research on the political culture of Russia and on the dynamics of reform as played out among the monarch, enlightened bureaucrats, conservative elder statesmen, abolitionist squires, and self-interested serfowners. Such studies have done much to enhance our understanding of the politics of reform, but they have not elucidated how specific law came into being.<sup>5</sup> They are confined to the political context of emancipation. They especially disregard a banking failure that plagued reform, warped legislation, and weakened subsequent economic development. In 1859, at the moment government officials sat down to write the reform laws, they faced a crisis—mounting state debt, inflation, a negative balance of payments, an unfavorable climate for foreign loans, the inability to reestablish convertibility of the ruble, and, finally, the collapse of state credit institutions.<sup>6</sup> During reform, the autocracy did not simply move “in stages” toward the final, politically balanced version of the emancipation laws. Nor does the view that serfowners successfully defended their economic interests, while not opposing emancipation proper, tell the whole story.<sup>7</sup> Discussion needs to focus on the constraints that confronted policy makers.

The banking crisis of 1859 is central. In late 1858, the government adopted as the cornerstone of its peasant reform policy the principle of a landed emancipation with compensation for the nobility. But an unexpected banking crisis only a few months later, while not lessening the government’s commitment to reform, greatly constrained reformers when they tried to turn policy into law. In fact, the terms of the 1861 land redemption law, as distinct from the political commitment to make the peasants property owners, cannot be understood without reference to the collapse of state credit institutions.

The banking crisis forced a sudden reordering of fiscal priorities. It prevented the government from subsidizing the serfs’ acquisition of their lands as in Prussia and Austria. It burdened the peasants for years with high interest rates on their redemption debts, substantially increasing their annual payments. It abetted those who wished to minimize the size of the peasants’ land allotments. It added weight

<sup>3</sup> M. N. Pokrovskii, *Russkaia istoriia s drevneishikh vremen*, 4 vols. (Leningrad, 1924), 4: 45–55; P. A. Khromov, *Ekonomicheskoe razvitiie Rossii v XIX–XX veka, 1801–1971* (Moscow, 1950), 7–25; P. A. Zaionchovskii, *Otmena krepostnogo prava v Rossii*, 3d edn. (Moscow, 1968), 62; I. D. Koval’chenko, *Russkoe krepostnoe krest’ianstvo v pervoi polovine XIX v.* (Moscow, 1967), 378–86; Daniel Field, *The End of Serfdom: Nobility and Bureaucracy in Russia, 1855–1861* (Cambridge, Mass., 1976), 29–33; R. E. F. Smith and David Christian, *Bread and Salt: A Social and Economic History of Food and Drink in Russia* (Cambridge, 1984), 340–41.

<sup>4</sup> Terence Emmons, ed., *Emancipation of the Russian Serfs* (New York, 1970), 4.

<sup>5</sup> Field, *End of Serfdom*; Terence Emmons, *The Russian Landed Gentry and the Peasant Emancipation of 1861* (Cambridge, 1968); W. Bruce Lincoln, *In the Vanguard of Reform: Russia’s Enlightened Bureaucrats, 1825–1861* (DeKalb, Ill., 1982); L. G. Zakharova, *Samoderzhavie i otmena krepostnogo prava v Rossii, 1856–1861* (Moscow, 1984).

<sup>6</sup> Jacob W. Kipp, “M. Kh. Reutern on the Russian State and Economy: A Liberal Bureaucrat during the Crimean Era, 1854–1860,” *Journal of Modern History*, 47 (September 1975): 447–48.

<sup>7</sup> Field, *End of Serfdom*, 5, 359.

to the view that redemption must be gradual, not immediate and mandatory. It necessitated the imposition of restrictions on the credit instruments issued to ex-serfowners to the disadvantage of the holders. All in all, the vast redemption operation and the harsh terms it imposed on the peasants were not simply the result of serfowners defending their interests. They were a response to fiscal constraints dictated by the collapse of state credit institutions.

IN RUSSIA, AFTER DEFEAT IN THE CRIMEAN WAR, the pressure for reform forced a revolution in public and private finance. And recent financial innovations in Western Europe did not go unnoticed. Overnight, a country that had never known securities trading created a stock and bond market. Domestically financed joint-stock companies, long prohibited, were suddenly permitted to organize. Chronic state budget deficits, for decades financed by borrowing from abroad or from monopolistic state banks, were now covered by floating internal treasury bonds. A century-old policy of liberal lending to serfowners for personal consumption was discarded. Transport development was made the investment priority, and the nobility was left without access to credit. All credit institutions of importance were abolished.

Throughout Europe, the 1850s saw intense financial innovation and expansion. Creation of the joint-stock bank *Credit Mobilier* in 1852 allowed France to overcome longstanding inadequacies in its capital markets that had hindered investment in public works and banking. Economic historians have hailed it "as a major innovation and discontinuity in the finance of not only France, but of Europe as a whole," for it soon had many imitators.<sup>8</sup> During this decade, the great German and Austrian investment banks were founded, providing capital for the boom in railroads as well as for mining and manufacturing. In Sweden, Italy, and Spain, several new banks were established. Even in England, with its sophisticated capital markets, this was a decade of considerable change. The Joint-Stock Companies Act of 1856 facilitated the formation of limited liability corporations and led to a spate of new finance companies. Of note to historians, it was these developments, especially the involvement of banks in the process of economic development, that underlay Alexander Gerschenkron's well-known theory of "relative backwardness."<sup>9</sup>

<sup>8</sup> Charles P. Kindleberger, *A Financial History of Western Europe* (London, 1984), 109; Rondo Cameron, *France and the Economic Development of Europe (1800–1914)* (Princeton, N.J., 1961), 134, 147–71.

<sup>9</sup> Kindleberger, *Financial History*, 12, 93, 112–24, 130–39, 148–49; Alexander Gerschenkron, *Economic Backwardness in Historical Perspective* (Cambridge, Mass., 1962); Alexander Gerschenkron, *Continuity in History and Other Essays* (Cambridge, 1968); Rondo Cameron, *et al.*, *Banking in the Early Stages of Industrialization* (Oxford, 1970), 107–09, 127, 162–63, 292; Rondo Cameron, *Banking and Economic Development: Some Lessons of History* (Oxford, 1972), 13–14, 38–39, 93–95, 104–05; Hugh Neuberger, *German Banks and German Economic Growth from Unification to World War I* (New York, 1977), 4, 64, 127; Holger L. Engberd, *Mixed Banking and Economic Growth in Germany, 1850–1931* (New York, 1981), 23–25; Gabriel Tortella-Casares, *Banking, Railroads, and Industry in Spain, 1829–1874* (New York, 1977), 13–14, 109, 180; Richard L. Rudolph, *Banking and Industrialization in Austria-Hungary: The Role of Banking in the Industrialization of Czech Crown Lands* (Cambridge, 1976), 69, 95; Richard H. Tilly, *Financial Institutions and Industrialization in the Rhineland, 1815–1870* (Madison, Wis., 1966).

Gerschenkron provided European economic historians with what still remains a useful framework for understanding state involvement in the early stages of industrialization under conditions of backwardness.<sup>10</sup> He pointed out the paradox of advantages in backwardness. The ability of late industrializers to borrow technological and institutional innovations at reduced cost from advanced countries was one such benefit. In emphasizing variability in the industrialization process, Gerschenkron described how backward nations mobilized capital differently from advanced ones. In Germany, investment banks took on the role previously played in England by the private entrepreneur. By contrast, "the scarcity of capital in Russia was such that no banking system could conceivably succeed in attracting sufficient funds to finance large-scale industrialization."<sup>11</sup> There, the state substituted its financial resources for the prerequisites the economy lacked.

Backward countries, however, are also apt to borrow policies, ideas, and institutions not well suited to their circumstances rather than to employ appropriate substitutes. In the late 1850s, Russia sought to create new financial institutions and credit instruments to address old problems it was finally willing to confront. But Russia at mid-century had only a primitive capital market. Borrowing ideas from Western finance capitalism was ill conceived, since the society lacked the capacity to use them effectively. Unlike the policy of state intervention adopted later in the century (which Gerschenkron describes), in the late 1850s the Russian autocracy, though hampered by the lack of national capital and developed financial markets, sought to rely on private, not state, resources. In doing so, the government precipitated a financial crisis, hitherto obscure, a consequence of a mania for private transport stocks.<sup>12</sup> In the end, the autocracy's attempt to provide for capital conflicted with its goal to create a free landed peasantry. Experimentation in Russia's immature capital markets at a crucial juncture in history had unfortunate consequences that persisted for decades. Two issues—the relationship of banks to economic development and the abolition of a servile system of labor—were linked in Russia.

STATE CREDIT INSTITUTIONS WERE ESTABLISHED in Russia in the second half of the eighteenth century to provide serfowners (*pomeshchiki*) with a cheap and reliable source of long-term credit. Essentially, the pre-1860 banking system consisted of four types of state credit institutions. Two were associated with welfare agencies,

<sup>10</sup> Gerschenkron, *Economic Backwardness*, 5–50; Alexander Gerschenkron, "The Discipline and I," *Journal of Economic History*, 27 (1967): 443–59; Cameron, *Banking and Economic Development*, 9–11; Paul Gregory, "Some Empirical Comments of the Theory of Relative Backwardness: The Russian Case," *Economic Development and Cultural Change*, 22 (1974): 654; Henry Rosovsky, "Alexander Gerschenkron: A Personal and Fond Recollection," *Journal of Economic History*, 39 (1979): 1012; Lars G. Sandberg, "Ignorance, Poverty, and Economic Backwardness in the Early Stages of European Industrialization: Variations on Alexander Gerschenkron's Grand Theme," *Journal of European Economic History*, 11 (1982): 675.

<sup>11</sup> Gerschenkron, *Economic Backwardness*, 19–20.

<sup>12</sup> For a definition of a financial crisis, see Charles F. Kindleberger, *Manias, Panics, and Crashes: A History of Financial Crises* (New York, 1978), 14–20; Julian Hoppit, "Financial Crises in Eighteenth-Century England," *Economic History Review*, 2d ser., 39 (1986): 39–41.



the Social Welfare Boards (*Prikazy Obshchestvennogo Prizreniia*) and the St. Petersburg and Moscow Savings Banks, which used their profits to support foundling homes and almshouses. In 1786, the State Loan Bank was restructured to increase the amount of long-term capital available to serfowners. Loans were not secured by the value of the land but by the number of serfs a landlord held. The term for these mortgage loans was frequently changed, although by the late 1850s most serfowner debt was for terms of twenty-eight or thirty-three years. A State Commercial Bank was set up in 1817 to provide short-term credit to merchants, although it remained an institution of little import. All state credit institutions were obligated to accept funds as demand deposits and pay interest on the accounts.

With long-term loans and demand deposits, the banking system was set up with a serious structural defect. In order to prevent a potential run on deposits, the government was forced to restrict the development of more profitable investment alternatives. Private commercial banks were prohibited, and the few joint-stock companies permitted to organize were required to raise their capital abroad.<sup>13</sup> This policy, coupled with regulations limiting the amount a serfowner could mortgage and prohibiting loans against small estates, often resulted in the state credit institutions holding surplus deposits on which they had to pay interest but for which they could find no borrowers. As a result, their profits were reduced considerably. In the 1820s, the state treasury solved the problem of accumulated idle capital by itself borrowing the money to cover budget deficits, thus eliminating the need to float internal public loans or to increase borrowing from abroad.<sup>14</sup>

In the wake of the Crimean War, the problem of surplus deposits resurfaced. To finance much of the war, the government resorted to printing money, over 400 million rubles, which more than doubled the amount of paper in circulation.<sup>15</sup> With no investment options available, paper soon accumulated in the state

<sup>13</sup> Iu. Hagemester, "O finansakh Rossii," *Tsentrāl'nyi Gosudarstvennyi Istoricheskii Arkhiv* (hereafter, TsGIA), f. 560, op. 43, d. 194, published in *Istoricheskii Arkhiv*, 2 (1956): 114–15; M. Kh. Reutern: Report to the Council of Ministers (September 16, 1866); published in *M. Kh. Reutern: Biograficheskii ocherk*, B. G. Reutern, et al., eds. (St. Petersburg, 1910), 72–73; E. Lamanskii, "Vklady v bankakh ili bilety nepreryvnogo dokhoda," *Russkii Vestnik*, 3 (1859): 223–38; E. Lamanskii, "Gosudarstvennye chetyrekhprotsentnye, nepreryvno-dokhodnye bilety," *Ukazatel' politiko-ekonomicheskii*, 14 (April 4–6, 1859): 320; E. L. [E. Lamanskii?], "Sovremennoe polozhenie kreditnykh uchrezhdenii v Evrope i Amerike i otnosheniia ikh k promyshlennym predpriiateliu," *Russkii Vestnik*, 2 (1857), book 1, 550–52; V. Sudeikin, *Gosudarstvennyi bank: Issledovanie ego ustroistva, ekonomicheskogo i finansovogo znachenii* (St. Petersburg, 1891), vii–xii, 90, 114, 134, 493; *Birzhevye Vedomosti*, 255 (November 29, 1862); *Narodnoe Bogatstvo*, 6 (November 7, 1862); E. Murav'ev, *Neskol'ko slov o novoi kreditnoi sisteme v sviazi s drugimi ekonomicheskimi voprosami* (Odessa, 1860), 14–15.

<sup>14</sup> *Ministerstvo finansov, 1802–1902* (St. Petersburg, 1902), chast' 1, 443; Hagemester, "O finansakh Rossii," 114; S. Ia. Borovoi, *Kredit i banki Rossii (seredina XVIII v.–1861 g.)* (Moscow, 1958), 178–80; A. Pogrebinskii, "Finansovaia reforma nachala 60-kh godov XIX veka v Rossii," *Voprosy Istorii*, 10 (1951): 78; [no author] "Finansovaia politika v periode 1861–1880 gg.," *Otechestvennye Zapiski*, 11 (1882), part 2, 11–12; *Pervoe izdanie materialov Redaktsionnykh Komissii, dlia sostavleniia "Polozhenii o krest'ianakh vykhodiashchikh iz krepstnoi zavisimosti."* 18 vols. (St. Petersburg, 1859–1860), 11: vedomost' 3 (hereafter, *PIM*); E. I. Lamanskii, "Iz vospominanii Evgeniia Ivanovicha Lamanskogo (1840–1890 gg.)," *Russkaia Starina*, 3 (1915): 580–81; E. L., "Sovremennoe polozhenie," 550–52; *Birzhevye Vedomosti*, 206 (August 4, 1867).

<sup>15</sup> During the war, the government was not able to cover its budget deficits by borrowing. Foreign capital markets were limited. Only 12 percent of total military expenditures were borrowed from abroad. While the state credit institutions held deposits of over 850 million rubles, much of their money was tied up in long-term loans to the state treasury and serfowners. Between 1854 and 1857,

credit institutions. By late 1857, they held over 180 million rubles of unused capital, far more than the 20 million rubles in cash they held on the eve of the war. Most disturbing, the government anticipated little treasury borrowing over the next two years. Serfowner demand was not expected to exceed 7 million rubles a year. The nobility had already pledged over 60 percent of its serfs, and many unencumbered estates were not eligible for mortgage.<sup>16</sup>

To reduce the losses that substantial idle capital would have caused the state credit institutions supporting welfare agencies and the small State Commercial Bank, the government permitted them to transfer funds to the State Loan Bank. In the 1856 annual report on the finances of the fifty-six Social Welfare Boards, the minister of interior noted: "Of the 126 million rubles which the boards have attracted, more than half remains unused, and a huge fund of 63 million rubles has been transferred to other [state] credit institutions, where it also has not been used . . . During 1856, total deposits grew by 15.5 percent, while loans increased by only 2.5."<sup>17</sup> Similarly, on January 1, 1858, the State Loan Bank held over 229 million rubles in deposit from the State Commercial Bank, which required less than 30 million rubles a year for its own banking operations. The State Loan Bank thus bore the burden of paying the 4 percent interest, over 6 million rubles a year by 1857, on the unused deposits. But ultimate responsibility fell on the treasury, for it guaranteed all deposits. Slowly, the treasury was being forced to pay for some of the paper it had issued to finance the Crimean War.<sup>18</sup>

In 1857, the government addressed a major problem that had underlain military defeat: the inadequacy of the transport system. In January, it announced "the construction of the first railroad network in Russia" and the formation of the giant Russian Railroad Company with an authorized capitalization of 275 million rubles. Much of the capital, in the form of stocks and bonds, was to be raised

218 million rubles were lent to the government by the various state credit institutions. On January 1, 1858, the treasury owed 521 million rubles to state credit institutions. Serfowner debt stood at 426 million rubles. *Ministerstvo finansov, 1802–1902*, chast' 1, 443; Lamanskii, "Vklady v bankakh," 222–24; Lamanskii, "Iz vospominanii," 3: 582–83; *Birzhevye Vedomosti* (June 2, 1862); "Finansovaia Politika," 4–6; N. Kh. Bunge, "Zametki o nastoiashchem polozhenii nashei denezhnoi sistemy i sredstvakh k ee uluchsheniiu," *Sbornik gosudarstvennykh znaniu*, V. P. Bezobrazov, ed. (St. Petersburg, 1880), 8: 106–08; Pogrebinskii, "Finansovaia reforma," 80; A. Pogrebinskii, *Ocherki istorii finansov dorevoliutsionnoi Rossii (XIX–XX v.)* (Moscow, 1954), 27–31.

<sup>16</sup> *Polnoe sobranie zakonov Rossiiskoi Imperii*, *Sobranie vtroe*, 55 vols. (St. Petersburg, 1830–94), vol. 34, no. 34,852 (September 1, 1859) (hereafter, *PSZ II*); *Otchet gosudarstvennykh kreditnykh ustanovlenii za 1859* (St. Petersburg, 1861), 4–5 (hereafter, *OGKU za* and the appropriate year); P. P. Migulin, *Nasha bankovaia politika (1729–1903)* (Kharkov, 1903), 61; I. S. Bliokh, *Finansy Rossii XIX stoletia*, 2 vols. (St. Petersburg, 1882), 2: 49; V. Sudeikin, *Gosudarstvennyi bank*, 137–46; V. Sudeikin, "Aleksandr Maksimovich Kniazhevich," *Russkaia Starina*, 11 (1892): 427–28; Hagemester, *TsGIA*, f. 661, op. 1, d. 97, l. 8. Cited in Pogrebinskii, *Ocherki istorii finansov*, 43; *PIM*, vol. 11, *vedomost' 3*; Iu. Hagemester, "Vzgliad na promyshlennost' i torgovliu Rossii," *Russkii Vestnik*, 1 (1857): 21–23; Lamanskii, "Vklady v bankakh," 223–24; Pogrebinskii, "Finansovaia reforma," 78–79.

<sup>17</sup> "Iz vlechenie iz vsepoddanneishego otcheta g. ministra vnutrennykh del za 1856 god," *Zhurnal Ministerstva vnutrennykh del*, 27 (1857): 48–49.

<sup>18</sup> N. Kh. Bunge, "O kreditnykh uchrezhdeniiakh v Rossii," *Zhurnal dlia Aktsionerov*, 89 (September 18, 1858); Lamanskii, "Vklady v bankakh," 222–23, 225, 228–29; Lamanskii, "Iz vospominanii," 3: 580–81; Hagemester, "O finansakh Rossii," 114; Reiter, *M. Kh. Reiter*, 76–78; Pogrebinskii, "Finansovaia reforma," 78–79. See also the *OGKU* for the appropriate year.

internally on St. Petersburg and Warsaw credit markets.<sup>19</sup> Subsequently, the government made clear its desire to facilitate the formation of other similar joint-stock ventures. As a result, between 1857 and 1859, it approved the formation of seventy-five joint-stock companies, mostly railroad and steam navigation firms. Altogether, they were permitted to raise over 140 million rubles.<sup>20</sup>

Because of financial crisis in much of Europe in 1857, initial stock offerings abroad went undersubscribed. To increase domestic incentive to invest in these firms, on July 20, 1857, the government announced a lowering of interest rates on deposits in state credit institutions. Interest rates on loans were also reduced.<sup>21</sup> The government made part of its reasoning clear in the statute itself: "In recent years, very substantial capital has begun to accumulate in state credit institutions, and these institutions, by virtue of their operations, are not able to give it proper use. Desiring to avoid the losses foreseen from this for the banking institutions and to give idle capital a use more in accordance with state interests, We deem it proper to lower the interest rate on deposits."<sup>22</sup> The interest rate on deposits of private individuals was reduced from 4 to 3 percent and the rate on all outstanding loans from 5 to 4 percent.

But other motives were behind this act as well. As a result of the war, debt service had become a considerable burden. In reducing the interest rate on loans to the state by 1 percent, the treasury would save over 5 million rubles a year. In addition, if the idle capital could be driven out of the banks, the government would save another 6 to 7 million rubles.<sup>23</sup> Thus the intent in lowering interest rates was to remove surplus capital from the state credit institutions, direct this money into joint-stock companies, and reduce government expenditures. In effect, the government manipulated interest rates to make up for an inefficient capital market, an act that was to cost it dearly for years to come. That this was made necessary by a financial crisis in Europe attests to the disadvantage of being dependent on foreign capital resources.

Initial response to the new interest rates was favorable.<sup>24</sup> At the first stockholders' meeting of the Russian Railroad Company on June 14, 1858, its president reported that "at present, the Russian public gives our undertaking its most eager support, offering its capital in amounts that exceed the expectations of the Company."<sup>25</sup> The same month, N. Kh. Bunge, then a professor of statistics and political economy in Kiev, wrote (in words he would later regret): "The lowering of bank interest rates from 4 to 3 percent is beginning to bear fruit . . . Fear

<sup>19</sup> PSZ II, vol. 32, no. 31,448 (January 26, 1857). See Alfred J. Rieber, "The Formation of La Grande Société des Chemins de Fer Russes," *Jahrbucher für Geschichte Osteuropas*, 2 (1973): 375–91.

<sup>20</sup> *Zhurnal dlia Aktsionerov*, 105 (January 8, 1859), and 194 (September 28, 1860).

<sup>21</sup> PSZ II, vol. 32, no. 32,082 (July 20, 1857). See also *Zhurnal dlia Aktsionerov*, 44 (October 29, 1857).

<sup>22</sup> PSZ II, vol. 32, no. 32,082 (July 20, 1857).

<sup>23</sup> Hagemester, TsGIA, f. 661, op. 1, d. 97, l. 10. Cited in Pogrebinskii, "Finansovaia reforma," 79; Lamanskii, "Iz vospominanii," 3: 583–84. See also Borovoi, *Kredit*, 275–76; and Migulin, *Nasha bankovaia politika*, 62–63.

<sup>24</sup> See Lamanskii, "Vklady v bankakh," 221–22, though Lamanskii himself did not hold such a view.

<sup>25</sup> *Ukazatel' ekonomicheskii* (July 6, 1858): 610–11.

concerning the withdrawal of deposits appears to be completely unfounded . . . We dare to assert beforehand that the lowering of interest rates has had the most happy results, assisting on the one hand the establishment of a proper balance between deposits and loans and on the other hand the development of domestic industry."<sup>26</sup> In June 1858, the new minister of finance, A. M. Kniazhevich, in his first speech to the Council of State Credit Institutions, reported that "the new regulations on deposits and loans . . . continue to be carried out without any difficulties."<sup>27</sup>

Since many stocks carried a 5 percent government guarantee compared to 3 percent paid on bank deposits, not only individuals but even the Social Welfare Boards withdrew substantial sums for investment in the stock market.<sup>28</sup> During 1858, withdrawals exceeded deposits by 50 million rubles.<sup>29</sup> The *Stockholders' Journal* reported in early 1858 that "on the St. Petersburg market, the prices of shares of all companies over the course of an entire year, and especially in recent months, have experienced an unparalleled increase. The main reason for this was the lowering of interest rates on capital in the state credit institutions, and as a result of this, the balance between the supply and demand for these shares has been destroyed."<sup>30</sup>

The only skeptic was Iu. A. Hagemeister, director of the Special Chancellery Credit Division. Hagemeister feared that "driving out deposits" from the state credit institutions would in fact lead to catastrophic withdrawals "ruinous for the treasury."<sup>31</sup> To meet demand, the government would be forced to print money or conclude a foreign loan on unfavorable terms.<sup>32</sup> As Hagemeister predicted, the situation deteriorated rapidly. By early 1859, it became clear to officials privy to the records of the state credit institutions that the banks would be unable to meet their obligations if the demand for withdrawals continued. Bunge wrote in late 1860 that "by the end of 1858, the crisis had become noticeable; by mid-1859, it was no longer a secret to anyone."<sup>33</sup> Similarly, E. I. Lamanskii, a key participant in resolving the banking crisis, recalled in his memoirs that "at the end of 1858 and the beginning of 1859, all the signs of a banking crisis had already appeared, which raised in turn the problem of a fundamental transformation of the then-existing banking system."<sup>34</sup> Eighteen months before, the banks were drowning in money. Now, insolvency threatened. Thus one of the first Great Reforms, the commitment of massive private resources to finance railroad construction and

<sup>26</sup> *Zhurnal dlia Aktsionerov*, 77 (June 26, 1858).

<sup>27</sup> *Ukazatel' ekonomicheskii* (September 7, 1858): 822; *Zhurnal dlia Aktsionerov*, 85 (August 21, 1858); later published in the *OGKU za 1857* (St. Petersburg, 1859), 4.

<sup>28</sup> Lamanskii, "Vklady v bankakh," 221–22, 234–35; Lamanskii, "Iz vospominanii," 3: 582–83 and 10: 58–59; Sudeikin, *Gosudarstvennyi bank*, 147–48; Bunge, "Zametki," 108–09; *Birzhevye Vedomosti*, 187 (June 15, 1863) and 136 (May 26, 1868).

<sup>29</sup> *OGKU za 1858*, 14.

<sup>30</sup> *Zhurnal dlia Aktsionerov*, 55 (January 16, 1858); see also 37 (September 10, 1857) and 75 (June 12, 1858).

<sup>31</sup> TsGIA, f. 661, op. 1, d. 97, l. 8, Report to the President of the Finance Committee of the State Council, cited in Pogrebinskii, "Finansovaia reforma," 79; A. Pogrebinskii, "Gosudarstvennye finansy Rossii nakanune reformy 1861 goda," *Istoricheskii Arkhiv*, 2 (1956): 101.

<sup>32</sup> Report to the President, TsGIA, f. 661, op. 1, d. 97, l. 10; Hagemeister, "O finansakh Rossii," 115.

<sup>33</sup> *Zhurnal dlia Aktsionerov*, 194 (September 28, 1860).

<sup>34</sup> Lamanskii, "Iz vospominanii," 3: 584–85.

steam navigation, brought about the necessity for another: the liquidation of the state credit institutions, the restructuring of their deposits, and the establishment of a new banking system.

KNIAZHEVICH, APPOINTED MINISTER OF FINANCE ONLY IN 1858, had not been involved in the decision the year before to lower interest rates, nor was he particularly experienced in financial affairs. One assessment of his four-year tenure at the ministry notes that, "while his name is not tied to one major piece of legislation," at least, in contrast to his predecessor, "he did not ruin matters." But Kniazhevich recognized the need to restructure existing bank deposits, and he appointed a number of men who were to dominate Russia's finances for the next three decades.<sup>35</sup> Bunge and M. Kh. Reutern, future ministers of finance, and Lamanskii, soon to be head of the new State Bank, all young men in their thirties, first rose to prominence through their involvement in resolving the banking crisis. Together with Hagemeister and N. A. Miliutin, former head of the economic department of the Ministry of Internal Affairs and now its assistant minister, they constituted five of the eight members on the commission to reform the banks, which would liquidate the state credit institutions and establish the State Bank.

Although often described as enlightened bureaucrats, these were men of narrow vision who saw the solutions to Russia's problems largely in fiscal terms. All were believers in hired wage labor, private banks, and prudent banking practices, of which Russia had little or none. They were as enamored of railroads as they were hostile to serfowners, whose profligate spending they disparaged. All desired tax reform, and they staunchly opposed deficit spending. Most had traveled widely in Europe, knew financial experts there, and were impressed with recent innovations in commercial credit.<sup>36</sup>

The first recommendation of this group was enacted on March 13, 1859, when the government announced the issue of 4 percent termless bonds, or perpetuities. This was the first of two attempts to restructure bank debt, that is, to convert demand deposits into long-term bonds. The government was less than candid and assiduously avoided the words crisis or insolvency in order to keep the markets calm. It merely declared that the goals of the interest rate reduction in 1857 had been achieved. Now it wished to address another concern. "The income of depositors who had not withdrawn their capital from the credit institutions had

<sup>35</sup> V. Sudeikin, "Aleksandr Maksimovich Kniazhevich," 416. See also Lamanskii, "Iz vospominanii," 4: 68–69.

<sup>36</sup> Lincoln, *In the Vanguard of Reform*; W. Bruce Lincoln, *Nikolai Miliutin: An Enlightened Russian Bureaucrat* (Newtonville, Mass., 1977); Kipp, "M. Kh. Reutern," 437–59; George E. Snow, "Introduction," *The Years 1881–1894 in Russia: A Memorandum Found in the Papers of N. Kh. Bunge; A Translation and Commentary* (Philadelphia, 1981), 10. N. Kh. Bunge, "Preobrazovanie nashikh kreditnykh ustanovlenii i likvidatsiia vkladov," *Zhurnal dlia Aktsionerov*, 164 (March 2, 1860); N. Kh. Bunge, "Ob izmenenii iuridicheskikh otnoshenii v Rossii, vsledstvie predstoiashchogo preobrazovaniia krepostnogo prava," *Otechestvennye Zapiski*, 8 (1858): 64; N. Kh. Bunge, "Bankovye zakony i bankovaia politika," *Sbornik gosudarstvennykh znani* (St. Petersburg, 1874), 1: 72–73; Lamanskii, "Iz vospominanii," 3: 575–77, 588 and 4: 63–69; Hagemeister, "O finansakh Rossii," 123–25; Hagemeister, "Vzgliad," 28; *Ukazatel' politiko-ekonomicheskii* (March 7, 1859), 231–32; Reutern, *M. Kh. Reutern*, 71–82, 100–02, 134–38; *Birzhevye Vedomosti*, 264 (December 11, 1862); 136, 138, and 143 (May 26, 28, June 2, 1868); Migulin, *Nasha bankovaia politika*, 93.



been reduced by one quarter, as a result of which many were left in a difficult position. Desiring to give depositors the possibility for a more profitable and equally safe investment, We confirm . . . the issuing of 4 percent state perpetuities.”<sup>37</sup>

A more realistic but still official picture appeared in the financial press. In the two months that followed, Bunge and Lamanskii published a number of articles encouraging individuals to purchase the new bonds. Careful not to cite any alarming figures, they both spoke of the problem posed by demand deposits and long-term loans, referring to recent withdrawals attendant to the formation of joint-stock companies. Lamanskii in particular condemned the lowering of interest rates in 1857 as a mere palliative that completely ignored “the main problem,” the structural defect in the banking system. Both explained that, although the new bonds did not involve government expenditures, the banks would have to forgo their profits. The 4 percent that serfowners paid on their loans would cover the 4 percent interest paid on the perpetuities. The benefit was that these bonds would conform better to the structure of existing long-term serfowner debt. The government pledged not even to raise the question of redeeming these perpetuities for twenty years.<sup>38</sup>

In spite of these efforts, the announcement of the 4 percent bonds resulted in panic and a run on deposits. Many people used their funds to purchase stocks, and the market soared higher. Others tried to send their money abroad. And the 4 percent perpetuities went undersubscribed.<sup>39</sup> In April, to reduce the amount of cash leaving the banks, the government suspended all new lending against serf estates and prohibited the rescheduling of any existing debt.<sup>40</sup> For the first time in over one hundred years, Russian serfowners were left without any source of institutional credit. The government explained that, with the impending emancipation and land settlement, future long-term loans would have to be based on land values, not the number of serfs.

At best, this was a factor of slight import, since it was clear that not enough bonds were being purchased to save the banks. In fact, over the next six months, only 13 million rubles of deposits were converted into perpetuities.<sup>41</sup> Within days of suspending loans to serfowners, the government committed itself in secret to reforming the banking system and liquidating the state credit institutions. Hagemeister, Bunge, Lamanskii, Reutern, and Miliutin were appointed to a formal commission on banking reform to draw up legislative proposals restructuring the banks.

In June 1859, Kniazhevich felt compelled to inform the Council of State Credit Institutions that the banks were in “rather difficult circumstances at the present

<sup>37</sup> PSZ II, vol. 34, no. 34,243 (March 13, 1859); published in *Zhurnal dlia Aktsionerov*, 116 (March 26, 1859). See also Bunge, “O kreditnykh uchrezhdeniiakh.”

<sup>38</sup> Lamanskii, “Vklady v bankakh,” 221–44; Lamanskii, “Gosudarstvennye chetyrekhpotsentnye,” 317–24; N. Kh. Bunge, “Nepreryvno-dokhodnye chetyrekhpotsentnye bilety,” *Zhurnal dlia Aktsionerov*, 120 (April 30, 1859); N. Kh. Bunge, “Luchshe li dlia gosudarstva imet’ potsentnyi dolg v 500 mil. rub. ili zhe na 500 mil. bumazhnykh deneg?” *Zhurnal dlia Aktsionerov*, 121 (May 7, 1859).

<sup>39</sup> “Finansovaia politika,” 13–14; Pogrebinskii, *Ocherki istorii finansov*, 42–44; *OGKU za 1859*, 6.

<sup>40</sup> PSZ II, vol. 34, no. 34,379 (April 16, 1859).

<sup>41</sup> PSZ II, vol. 34, no. 34,852 (September 1, 1859).

time.” Reported in the financial press, this only increased withdrawals.<sup>42</sup> The deteriorating financial situation necessitated a number of interim measures. To cover the demand for withdrawals, the government authorized the issuing of 100 million rubles, of which 88 million were actually put in circulation, and a loan of 7 million pounds sterling was obtained.<sup>43</sup> Existing deposits in the banks were divided into four categories, with various treasury and other state funds no longer to receive interest.<sup>44</sup> The formation of new joint-stock companies was halted. In July 1859, the Social Welfare Boards and the St. Petersburg and Moscow Savings Banks ceased to be independent banking entities and were made subordinate to the Ministry of Finance. Thus, in another major reform, credit and banking policy were for the first time in modern Russian history separated from charitable agencies.<sup>45</sup>

On September 1, 1859, in a state of utter panic, the government announced the second and significantly more costly attempt to convert demand deposits into long-term notes. Five percent bank notes, to be liquidated over a period of thirty-seven years, were to be issued.<sup>46</sup> The government was now lending money for less than the cost of borrowing. In other words, serfowners were paying 4 percent on their loans to the banks, which in turn paid out 5 percent to bondholders. To entice, or perhaps force, depositors into conversion, the interest rate on bank deposits was lowered again, to 2 percent. It was now half of what it had been two years before. Appended to the statute was an unusually candid discussion of the need for banking reform.<sup>47</sup>

In his report, Minister Kniazhevich discussed the evolution of government banking policy:

The structure of our main state banking institutions, the basis of which was established as far back as the previous century, no longer corresponds to present-day circumstances and requires fundamental reform . . . This has been made especially noticeable of late, with the revival of industrial entrepreneurship, by the anticipated building of railroads and other improved systems of transport, and finally in light of the great economic reform, being prepared in accordance with the ideas and instructions of Your Imperial Highness, for a large part of the rural population.<sup>48</sup>

He described the problem of idle capital and the unfortunate consequences of lowering interest rates in 1857. The careful reader could see that as of July 1, 1859, the banks had on hand only 23 million rubles in cash. In contrast, excluding state deposits, private institutional endowments, and other restricted funds, 365 million rubles “could be demanded at any time.” And these demand deposits

<sup>42</sup> *Zhurnal dlia Aktsionerov*, 29 (July 2, 1859); later published in *OGKU za 1858* (St. Petersburg, 1860), 14.

<sup>43</sup> *OGKU za 1859*, 12; Reiter, *M. Kh. Reiter*, 10; Sudeikin, *Gosudarstvennyi bank*, 148–49; Sudeikin, “Aleksandr Maksimovich Kniazhevich,” 427–28; “Finansovaia politika,” 13–14; Pogrebinskii, *Ocherki istorii finansov*, 42–44, 65; Borovoi, *Kredit*, 278–79.

<sup>44</sup> *PSZ II*, vol. 34, no. 34,692 (June 29, 1859).

<sup>45</sup> *PSZ II*, vol. 34, no. 34,730 (July 10, 1859).

<sup>46</sup> *PSZ II*, vol. 34, no. 34,852 (September 1, 1859).

<sup>47</sup> Published in *Zhurnal dlia Aktsionerov*, 142 (September 17, 1859); *Ukazatel' politiko-ekonomicheskii*, 38 (September 18, 1859); *Zemledel'cheskaia Gazeta*, 77 (September 25, 1859). See also *Proizvoditel' i Promyshlennik*, 62–63 (September 19 and 22, 1859).

<sup>48</sup> *PSZ II*, vol. 34, no. 34,852 (September 1, 1859).

"could create difficulties . . . extremely burdensome for the government." Therefore, credit had to be placed "on a more rational basis."<sup>49</sup>

Kniazhevich revealed the full picture only a year later, after a number of reforms had been enacted. "From August 1857 over the course of the next twenty-two months, the withdrawal of deposits exceeded new deposits by 143 million rubles, and the cash on hand in the banks, which in June 1857 exceeded 150 million rubles, had fallen by June 1859 to 20 million rubles." Kniazhevich explained that he had foreseen for the remainder of 1859 an immediate demand of up to "50 million rubles for corporate capital," with the threat of substantially greater withdrawals if existing joint-stock companies issued all the shares they were authorized to sell. He again spoke of the banks' "difficult situation." In reality, they had been insolvent, a word that could be used only after the fact.<sup>50</sup>

Upon Kniazhevich's retirement as minister of finance in 1862, *Stock Exchange News* (*Birzhevye Vedomosti*) described his efforts "to protect the banks from obvious insolvency."<sup>51</sup> His successor, Reutern, in documenting for the tsar and the Council of Ministers the reasons for the persistent problems in Russia's credit market, explained that, by lowering interest rates, "the credit institutions were made insolvent and could continue to make payments only by the issuing of new paper notes." In fact, he asserted, "not only did the credit institutions turn out to be insolvent but so, it could be said, did all of Russia."<sup>52</sup>

Fearful of failing a second time to get depositors to invest in long-term bonds and, consequently, having to print even more money or, worse, suspend the withdrawal of deposits, the Banking Commission sought to make the new 5 percent bank notes extremely attractive. Lamanskii ascribed the failure of the 4 percent perpetuities issued in March 1859 to the fact that no provision was made for their liquidation. Investors, not knowing when the capital would be returned, he believed, were not adequately compensated by the 1 percent interest above the rate on demand deposits.<sup>53</sup> Thus the new bonds not only carried a higher interest rate, 5 percent, but buyers could specify the term of bond they desired.

Also, the 5 percent bank notes were designed to be considerably more attractive than most stock investments. The bonds came with tear-off coupons to facilitate the receipt of interest payments, which could be obtained at any branch of the Commercial Bank and all 600 county treasury bureaus. In contrast, stock dividends were often difficult and costly to obtain for those living outside of St. Petersburg. Provisions were made for the bonds to circulate abroad, and they could be sold or transferred by simple endorsement, much more readily than stock. In addition, they could be mortgaged at up to 90 percent of their value in the Commercial Bank or any office of the Social Welfare Boards, a privilege shared only with stockholders of the Russian Railroad Company. Other stocks

<sup>49</sup> PSZ II, vol. 34, no. 34,852 (September 1, 1859).

<sup>50</sup> For the text of the speech to the Council of State Credit Institutions, see *OGKU za 1859* (St. Petersburg, 1861), 4–5; *Ukazatel' ekonomicheskii*, 40 (October 1, 1860); *Aksioner*, 38 (September 23, 1860); *Zhurnal dlia Aksionerov*, 192 (September 14, 1860).

<sup>51</sup> *Birzhevye Vedomosti*, 255 (November 29, 1862).

<sup>52</sup> Reutern, M. Kh. *Reutern*, 74, see also 134. Twenty years later, Bunge admitted that "after the mistake was made to lower interest rates on bank deposits," a "feverish activity in industry set in, which ended with the exhaustion of the banks' cash reserves"; Bunge, "Zametki," 108–09, 114.

<sup>53</sup> Lamanskii, "Iz vospominanii," 3: 584–85.

could not be accepted as collateral. Later, the government ordered that these bonds be accepted at nominal value when used to pay off existing mortgage debt.<sup>54</sup> In sum, banking reform was now being given a higher priority than even railroad and steam navigation development, a shift in policy that the stock market was soon to recognize.

Only holders of unrestricted deposits in the state credit institutions were allowed to purchase the new bank notes. Restricted or institutional deposits could not be used, though these funds could still be invested in the 4 percent perpetuities issued in March. An early deadline of December 31, 1859, was set for the conclusion of the conversion, as there was considerable concern about a protracted crisis.<sup>55</sup> After that date, depositors choosing to leave their money in the banks as demand accounts would receive only 2 percent interest.

At first, response to the new 5 percent bonds was slow. Because this was a form of investment new to Russia, many feared the bonds would not be redeemed as promised. And, it seems, the administrators of the state credit institutions were not sympathetic to the reform. Lamanskii recounted in his memoirs that "Minister of Finance Kniazhevich was in complete despair." Members of the Banking Commission began to blame each other for the reform "not coming up to expectations."<sup>56</sup>

As they had during the previous spring, Bunge and Lamanskii launched a campaign in the press to encourage investment in the new bonds. They stressed that the restructuring of existing deposits was a necessary prelude to a complete reform of the banking system. The recent consolidation of all banking operations under the Ministry of Finance, they advised, would ensure accurate and proper accounting, not only of the new bonds but of all state deposits and debts. Both Bunge and Lamanskii were straightforward about the costs. With the state credit institutions charging borrowers 4 percent and the new bonds paying 5 percent on the converted deposits, the annual loss, Bunge guessed, would run approximately 6.75 million rubles a year. Lamanskii was less specific, stating only that "initially this reform would be fairly costly; but, as can be seen from the minister's report, the treasury is prepared for these expenses."<sup>57</sup>

The deadline for purchasing the 5 percent bonds had to be extended, but the restructuring proved successful. By September 1860, 272.6 million rubles of deposits had been used to purchase the thirty-seven-year notes. The lowering of interest rates on demand deposits to 2 percent, which took effect on January 1, 1860, encouraged many institutions to buy 4 percent perpetuities. By late 1860, almost 148 million rubles of restricted and institutional deposits were converted into perpetuities or had been pledged to be converted. Thus over 43 percent of all deposits in the state credit institutions were exchanged for long-term bonds. Cash withdrawals were covered by the issuing of paper money and two foreign loans, including one on rather unfavorable terms. The first year's cost of the

<sup>54</sup> PSZ II, vol. 37, no. 38,490 (July 23, 1862).

<sup>55</sup> Lamanskii, "Iz vospominanii," 4: 63.

<sup>56</sup> Lamanskii, "Iz vospominanii," 4: 66.

<sup>57</sup> E. I. Lamanskii, "Neskol'ko slov ob"iasnenii po povodu piatiprotsentnykh bankovykh biletov," *Ukazatel' politiko-ekonomicheskii*, 39 (September 26, 1859); N. Kh. Bunge, "Preobrazovanie russkikh bankov," *Zhurnal dlia Aktsionerov*, 141 (September 24, 1859).

restructuring was 10.1 million rubles, considerably higher than Bunge's estimate.<sup>58</sup> Total costs over the life of the bonds were expected to approach 200 million rubles.

One neglected aspect of the Great Reforms is this massive shift of capital out of the state credit institutions. Because their records were not public and the state did not publish its budget, few persons outside the government's small circle of financial advisers were aware of the true dimensions of the crisis until late 1860, almost a year after it had passed. The official explanation for the new banking legislation served to play down, if not cover up, the crisis. But, in 1859, the government was not merely seizing an opportune moment for banking reform, it was confronted with the imminent collapse of the state credit institutions. Within six months, they were all abolished. A new State Bank assumed their assets and liabilities, including the heavy annual losses resulting from having to liquidate the 5 percent bank notes.<sup>59</sup>

THE IMMEDIATE CRISIS WAS THUS RESOLVED. But, during the spring and summer of 1859, debt conversion, transport development, peasant land reform, and agricultural credit became intertwined. It would be useful to review the sequence of events to see how the banking crisis constrained the government's handful of financial advisers. In December 1858, as the financial crisis was becoming evident, the government committed itself to a landed emancipation. In February 1859, the tsar ordered the formation of an Editing Commission (actually two, although only one was formed) to draft the legislation. This commission began its work in March. Simultaneously, the government's coterie of financial advisers struggled with the banking crisis, leading to the March issue of 4 percent perpetuities. By April, it was clear that the introduction of these termless bonds had only aggravated the problem. The state credit institutions were on the verge of collapse, and a formal Banking Commission was established. On April 27, a special Finance Commission was formed to write the peasant land redemption law, as this required expertise not possessed by members of the Editing Commission. The Banking Commission and the Finance Commission each consisted of eight persons. Bunge, Lamanskii, Hagemeister, Reutern, and N. A. Miliutin served on both. Out of necessity, banking reform took precedence. On July 10, the government announced yet another panel, the Commission on the Establishment of Land Banks, charged with drafting legislation to reestablish long-term agricultural credit. Landlords had been without access to credit since April. This fourteen-member commission included a number of legal experts. But, for financial matters, the government called upon the same five men and A. P. Zablotskii-Desiatovskii, former director of the agriculture department of the

<sup>58</sup> *OGKU za 1859*, 5–12; *Zhurnal dlia Aktsionerov*, 6, 92 (February 8 and September 14, 1860); *Aktsioner*, 38 (September 23, 1860); *Ukazatel' ekonomicheskii*, 4, 89 (October 1, 1860 and November 12, 1861); Reutern, *M. Kh. Reutern*, 148–49; *Birzhevye Vedomosti*, 10, 164, 255, 38 (January 13, 1861, August 3 and November 29, 1862, and May 28, 1868); "Finansovaia politika," 13–14; Sudeikin, *Gosudarstvennyi bank*, 148–49; Migulin, *Nasha bankovaia politika*, 78–87; Borovoi, *Kredit*, 278–79; Pogrebinskii, *Ocherki istorii finansov*, 30–31.

<sup>59</sup> *PSZ II*, vol. 34, no. 35,287 (December 26, 1859).



Ministry of State Domains and also a member of the Finance Commission. Hagemeister was appointed chair.

The Finance Commission spent much of the spring and summer preparing the first draft of the redemption law, which was completed on August 12. On August 25, the Commission on Land Banks began its work, which continued until December. By then, it seemed certain that the second attempt at debt conversion would succeed. The Banking Commission turned to writing the charter for the new State Bank. On July 1, 1860, with a figurehead as director and Lamanskii as vice-director, the State Bank opened. The government's financial experts continued their work on the redemption law throughout 1860. In late November, it was ready for approval.

The question was, could these men successfully develop a coherent policy to meet Russia's pressing transportation, agrarian reform, and banking needs? In September 1859, just after the first draft of the redemption law was completed and the second conversion of bank deposits had been announced, Bunge and Lamanskii addressed this issue in order to quell concern. To those who worried whether Russia possessed the financial resources both to convert the bank deposits and fund peasant land reform, Bunge said it did. He expressed reservations, however, about whether sufficient capital would be available to provide the nobility with new forms of long-term credit to restructure their estates using hired wage labor.<sup>60</sup> Lamanskii was more optimistic. "There is no longer any reason to fear either the development of industrial enterprises, the issuing of obligations for the important social reform of peasant life, or justifiable borrowing by the government in periods of extraordinary need, in a word, all the things which we need and to which, until now, our credit institutions have continuously served as an obstacle."<sup>61</sup> As for rural land credit, he asserted, "Mortgage loans against immovable property will soon develop and in even greater amounts than before, but on other, more solid bases, safer for the banks, and under conditions just as advantageous for borrowers."<sup>62</sup> But Lamanskii was mistaken.

To many observers and officials, undertaking a massive redemption operation that required vast amounts of new credit so soon after the issue of 275 million rubles of easily circulating bonds was not possible. But the members of the Finance Commission expended considerable effort to tailor the redemption law to the banking crisis.<sup>63</sup> Initially, the government planned to extend substantial financial assistance to the redemption operation. In February 1859, Ia. I.

<sup>60</sup> Bunge, "Preobrazovanie russkikh bankov."

<sup>61</sup> Lamanskii, "Neskol'ko ob"iasnenii," 875–76.

<sup>62</sup> Lamanskii, "Neskol'ko ob"iasnenii," 875–76.

<sup>63</sup> Resolution of these matters was left almost solely to the Finance Commission. According to P. Kovan'ko, a nineteenth-century historian, "neither the Secret Committee nor its successor (in name only) the Main Committee [of Peasant Affairs] had the slightest idea of how to accomplish an emancipation of the peasants under the condition of allotting them land. Both to the Committee, and to Rostovtsev [its chairman], this financial operation represented something fearful, and scarcely practicable." The proposals of the Finance Commission were not reviewed by the Editing Commission, as was done with other emancipation legislation. They were little changed by higher government bodies. The Main Committee and the Ministry of Finance began their review of the draft law only in late November 1860. The draft statute reached the State Council just ten days before it was enacted in February 1861. P. Kovan'ko, *Reforma 19 fevralia 1861 goda i ee posledstviia s finansovoi tochki zreniia (Vykupnaia operatsiia 1861 g.—1907 g.)* (Kiev, 1914), 3–4, 156–58, 171–74.

Rostovtsev, chairman of the Editing Commission, suggested that as much as 15 million rubles a year might be available. In fact, prior to the formation of the Editing Commission, special legislation was passed, and then renewed in 1859 and 1860, that designated certain revenues from the Ministry of State Domains for emancipation purposes, though not necessarily for the redemption operation.<sup>64</sup> By the time the Finance Commission began its work in mid-1859, Rostovtsev had already lowered his figure of potential government assistance to 5 million rubles.<sup>65</sup> But the members of the Finance Commission were adamant that the redemption operation be a pure credit transaction, completely self-supporting. In the process of committing substantial government resources for thirty-seven years to restructure the deposits in the state credit institutions, the commission members opposed any new government expenditures except for railroad development.<sup>66</sup> The commission was firm on the question of government assistance. "The aim, which the government is able to take upon itself to guarantee . . . consists strictly in expediting the peasants' purchase of land and in ensuring former serfowners the proper receipt of payments owed; it is impossible to go beyond this: obviously at the present time the government cannot take upon itself the obligation to provide financial resources, at any price, to make the [peasants] property owners; to do otherwise would mean subjecting the entire operation to an unaccountable risk."<sup>67</sup> As the commission's report succinctly stated, "By way of financial prudence, it should be taken as a principle that the redemption operation should, so to speak, sustain itself, and not burden the state treasury with new permanent expenditures."<sup>68</sup> Peasant payments were to cover all interest and principal, all administrative costs, and all contingencies. No funds were ever appropriated for the redemption operation itself.<sup>69</sup>

By comparison, in Prussia, the government paid a significant portion of the redemption loan principal, covered all administrative costs, and made it possible to liquidate the debt even faster, if certain funds became available. In Austria, government assistance was even greater. The Finance Commission discussed these redemption acts at length, since many expected the Russian redemption operation to be modeled on Prussian and Austrian precedents. But the commission found it necessary to limit the government's role to that of a mediator, not an underwriter.<sup>70</sup> The tsarist government did not spend a kopek in undertaking the Great Reform of making twenty million peasants property owners.

Such financial prudence cost the peasants dearly. The redemption law specified that once peasants became property owners (and no provision was made requir-

<sup>64</sup> Ia. I. Rostovtsev, "Khod i iskhod krest'ianskogo voprosa," signed on February 14, 1859, and published in *PIM*, vol. 1, pp. 58–59; *PIM*, vol. 11, pp. 93–96. See also his comments to the full Editing Commission, Journal of the Editing Commission, *PIM*, vol. 1 (March 5, 1859), p. 5; (April 29, 1859), pp. 36–37.

<sup>65</sup> *PIM*, vol. 1 (May 20, 1859), pp. 48–50.

<sup>66</sup> Hagemeister, "O finansakh Rossii," 123–25; Hagemeister, "Vzgliad," 1: 28. See also *Birzhevyie Vedomosti*, 264 (December 11, 1862).

<sup>67</sup> *PIM*, vol. 11, p. 138.

<sup>68</sup> *PIM*, vol. 11, p. 217.

<sup>69</sup> Kovan'ko, *Reforma*, 27. Of the 23.5 million rubles appropriated for emancipation, most of it was spent by the government to purchase estates from serfowners whose lands did not qualify for redemption under the terms of the general statute.

<sup>70</sup> Kovan'ko, *Reforma*, 105.

ing this be done), a forty-nine-year credit operation would begin. The government would compensate serfowners for the land given to the peasants with 5 percent bank notes (bonds) and 5 percent redemption certificates. The bank notes, designated second series to distinguish them from the thirty-seven-year bonds issued to restructure bank deposits, were to be liquidated in series. The second series bank notes carried all the same privileges as the first. As for the certificates, every five years from the date of each redemption agreement, one-third were to be exchanged for bank notes (bonds), until, after fifteen years, a landlord no longer held any certificates. All obligations were to be liquidated within the forty-nine-year term.<sup>71</sup> In 1862, it was announced that 5.5 percent annuities were to be issued for lands initially not eligible for redemption. The government promised to announce the terms of their liquidation within ten years, although these annuities were also to be redeemed within forty-nine years of their date of issue.<sup>72</sup>

The peasants, in turn, were to pay the government 6 percent annually of the total redemption sum given their former owner. Of this, 5 or 5.5 percent was to cover the interest on the obligations issued and .5 percent to pay off the principal. The remaining .5 percent from the bank notes and redemption certificates was for administrative costs and defaults. In other words, on that portion of a redemption grant issued in 5 percent paper, 1 out of every 12 rubles the government received back from the peasants went not to pay interest or principal but to cover administrative expenses and contingencies. And most redemption grants paid 5 percent. Between 1862 and 1866, 62 percent of all redemption paper was in 5 percent obligations. After 1874, 5.5 percent paper was no longer issued. Through January 1, 1885, 78 percent of total redemption obligations paid 5 percent.<sup>73</sup> On over three-fourths of all peasant payments, the government was receiving 8.5 percent beyond what was needed to cover the interest and principal. This margin more than ensured that the treasury did not have to subsidize the redemption operation.<sup>74</sup> In other words, peasant redemption debt carried high interest rates and high reserve requirements. The impact of the banking crisis on market interest rates and on the government's ability to subsidize land redemption was the cause.

By way of ensuring even further that the redemption operation did not lose money, the Finance Commission imposed restrictions common to mortgage transactions. Under the terms of the redemption law, the compensation to landlords—and thus the size of peasant payments on this debt—was not to be

<sup>71</sup> *PSZ II*, vol. 36, no. 36,659, articles 114, 140–55 (February 19, 1861).

<sup>72</sup> *PSZ II*, vol. 37, no. 38,407 (June 27, 1862).

<sup>73</sup> *Otchet gosudarstvennogo banka po vykupnoi operatsii s otkrytiia vykupa po 1 ianvaria 1892 goda* (St. Petersburg, 1893), 48.

<sup>74</sup> Although the figures cannot be considered very reliable, various estimates place the surplus by 1877, after all administrative costs and accumulated arrears were deducted, between 20 and 65 million rubles. I. Vil'son, "Vykupnye za zemli platezhi krest'ian-sobstvennikov byvshikh pomeshchich'ikh, 1862–1876," *Zapiski Imperatorskogo Russkogo Geograficheskogo Obshchestva po otdeleniiu statistiki* (St. Petersburg, 1878), 5: 315–16; Kovan'ko, *Reforma*, 9, 11, 274–77, 434–35; P. P. Migulin, *Vykupnye platezhi k voprosu o ikh ponizhenii* (Kharkov, 1904), 13; [no author] "Obshchii balans vykupnoi operatsii," *Vestnik Finansov*, 23 (1903), prilozheniia, 12. See also *Birzhevyi Vedomosti*, 218 (May 19, 1870).

determined by a valuation of the land to be redeemed. To do so would have necessitated compilation of a cadastre, delaying the reform for years. In this case, Prussian and Austrian models provided the solution. Quitrent (*obrok*), in effect, the peasants' ability to pay, became the basis for assessment. If peasants and landowners could not reach an agreement on the amount of quitrent, various regional norms were to be used. The quitrent was multiplied by a factor of 16.67 to obtain the proximate value of the land, that is, quitrents were capitalized at 6 percent.

A dilemma arose, however, because the actual land the peasants received, which served as collateral for their debt to the government, did not always correspond to its capitalized value. The Finance Commission recognized that, while there was a strong correlation between quitrent and land values, the relationship was not perfect. This gap could expose the government to unjustifiable risk. For example, the commission assumed that in regions where non-agricultural income was common, serf quitrent probably reflected these revenue sources.<sup>75</sup> The Finance Commission responded to the situation in the way any modern bank would; it refused to lend the peasants, or grant the landlord, the full valuation sum. Instead, a redemption grant was limited to 75 or 80 percent of the capitalized quitrent, depending on various circumstances. Thus, as the Finance Commission explained, "for greater prudence and by way of eliminating any risk to State credit," it was necessary to provide for a 20 or 25 percent margin on redemption collateral.<sup>76</sup>

THE MOST DIFFICULT PROBLEM for the Finance Commission became how to make the peasants property owners through a credit operation of 800 million rubles without endangering Russia's already strained credit markets. Fear was widespread in government circles that the redemption operation, even if it did not require government funds, was dangerous.<sup>77</sup> Lamanskii did not share these concerns. But, in his memoirs, he recorded that "the other members of the commission, including M. Kh. Reutern, feared that the appearance in circulation of such a mass of interest-bearing paper would destroy the money market, especially given the general unfamiliarity in Russia with large credit operations. These fears served, by the way, as a basis for all sorts of restrictions, at least at first, on redemption transactions."<sup>78</sup> In 1866, Reutern described his fears: "the Great Reforms . . . found us with a system of credit institutions that was ruined, chaotic monetary circulation, and inadequate national capital."<sup>79</sup> The solution of the Finance Commission was to have redemption be a slow, gradual, and voluntary process. The amount of credit paper issued was limited and, once issued, hampered in its ability to enter the market.

<sup>75</sup> *Vtoroe izdanie materialov Redaktsionnykh Komissii, dlia sostavleniia "Polozhenii o krest'ianakh vykhodiaschikh iz krepostnoi zavisimosti"* (St. Petersburg, 1859–60), vol. 1, book 1, 160. See also Kovan'ko, *Reforma*, 137.

<sup>76</sup> *PIM*, vol. 11, pp. 157–61; *PSZ II*, vol. 36, no. 36,659 (February 19, 1861), article 66.

<sup>77</sup> Kovan'ko, *Reforma*, 2–3; Migulin, *Vykupnye platezhi*, 1.

<sup>78</sup> Lamanskii, "Iz vospominanii," 3: 586–87.

<sup>79</sup> Reutern, *M. Kh. Reutern*, 135. See also *Birzhevyi Vedomosti*, 274 (December 22, 1862).

Austrian and Prussian precedents of an immediate, mandatory redemption of peasant holdings were not followed in Russia. Rostovtsev, echoing the tsar's views, argued it would be "unjust" to force landowners to sell and peasants to buy land at fixed government prices.<sup>80</sup> But, in order to ensure that the tsar's views and those of his leading advisers did not change, as they did on many issues between 1857 and 1861, the Finance Commission added its weight to the principle of a voluntary or, at least, gradual redemption. Referring to Austria and Germany, the commission emphasized, "*In general, mandatory redemption was usually a consequence of political necessity that demanded decisive measures for the quieting of the rural population.*"<sup>81</sup> This, the commission argued, was not the case in Russia. Of more direct financial concern, the commission warned the government, "Compulsory redemption would require the sudden issue of credit paper in huge amounts, which would give rise to very ruinous consequences both for internal credit and for the landowners themselves."<sup>82</sup> Worried about flooding the market with redemption paper so soon after the issue of first series bonds, the commission asserted that "protecting internal credit from all sorts of fluctuations, even if only temporary, and from even the slightest risk, will necessitate certain measures for keeping redemption transactions within the bounds of strict, gradual development."<sup>83</sup> Finally, the commission noted that many serfs on *corvée* (*barshchina*) did not have the ability to earn sufficient cash income to meet redemption payments. In its view, this was yet another obstacle to immediate, universal, mandatory redemption.<sup>84</sup>

As a result of these concerns, the law did not extend redemption to peasants on *corvée*, and it limited the size of an allotment the government would take in mortgage. Article 30 of the redemption law stated: "Assistance from the government in the acquisition by the peasants of their allotment as property is extended only to peasants on quitrent." Many provincial committees, in reviewing the draft legislation, expressed their opposition to *corvée* estates being ineligible for redemption. The commission responded: "Indeed, the regulation cited gravely limits the application of the redemption operation, as at present scarcely a third of the serf population is on quitrent, and most probably the abolition of *corvée* will take place rather slowly, owing to inadequate cash resources of the peasants . . . . Meanwhile, the status of *corvée* estates under the new order will be particularly difficult for the owners, the residents, and the government."<sup>85</sup> Numerous serfowners suggested that measures be adopted to permit the redemption of *corvée* estates. But the commission was unmoved. "In spite of the importance of such arguments, the members of the Finance Commission do not see any possibility either to begin the redemption of *corvée* estates, or to facilitate, by some sort of artificial measures, the transfer of peasants onto quitrent."<sup>86</sup> The

<sup>80</sup> *PIM*, vol. 1, p. 59; vol. 3, p. 13.

<sup>81</sup> *PIM*, vol. 11, p. 133, emphasis in the original.

<sup>82</sup> *PIM*, vol. 11, p. 136.

<sup>83</sup> *PIM*, vol. 3, pp. 15–16, 26.

<sup>84</sup> *PIM*, vol. 3, p. 27; vol. 11, pp. 146–48; Kovan'ko, *Reforma*, 18–19.

<sup>85</sup> *PIM*, vol. 11, p. 146.

<sup>86</sup> *PIM*, vol. 11, p. 147.



principle of a gradual redemption process to limit the flow of credit paper onto the market was enacted into law.<sup>87</sup>

This same consideration gave rise to restrictions on the physical size of an allotment that the government would accept in mortgage. Prior to receiving the preliminary report of the Finance Commission, the Editing Commission, following the advice of the minister of the interior and his land department, came out strongly in favor of the peasants receiving their existing allotments. "It stands to reason that designated limits ought to be established within such wide boundaries so that cutting off surplus peasant land will not be a general phenomenon," the economic division of the Editing Commission wrote in its very first report.<sup>88</sup> But, as is well known, reductions in the size of peasant landholdings were common under the terms of the emancipation laws.<sup>89</sup> For this, the Finance Commission bore some responsibility, though other factors, especially the desire to maintain agriculture among the gentry, were of greater significance.

Redemption was to be largely a voluntary agreement, not mandatory or immediate, with landowners and peasants themselves determining when to proceed, the size of the allotment, and the value of the land. The government provided norms only if an agreement could not be reached and the landlord insisted on redemption.<sup>90</sup> In response to this policy of voluntary agreements, the Finance Commission cautioned, "It is necessary to establish regulations for determining the maximum size of the redemption sum allowed by the government."<sup>91</sup> The commission feared that a voluntary process with no limit on the amount of land both parties might subject to redemption could lead to a far larger redemption operation than the government desired and to the uncontrolled issue of credit paper. Moreover, the sheer size of the peasants' debt, even if adequately secured, might endanger both state finances and private credit.<sup>92</sup>

Thus, in contrast to the Editing Commission, which in its early deliberations desired that most peasants keep their existing allotments, the Finance Commission sought to limit the maximum allotment out of fiscal considerations. Throughout 1859 and 1860, the Editing Commission was pressured by many groups, including the Finance Commission, to reduce the maximums, and eventually this was done. Under the redemption law, landlords and peasants were free to reach any agreement on the amount of land the peasants were to acquire and the price they were to pay. But the maximum amount of land that could be purchased through the government's credit operation was limited. Similarly, maximum quitrents were set for computing the capitalized value of the land. The allotment and quitrent norms used were the same ones the government established in the event that a voluntary agreement could not be reached and the landlord then forced redemption.<sup>93</sup>

<sup>87</sup> Kovan'ko, *Reforma*, 116, 137–39, 147.

<sup>88</sup> *PIM*, vol. 2 (June 9, 1859), pp. 9, 12–13, 27.

<sup>89</sup> Zaionchkovskii, *Otmena krepostnogo prava*, 237–42.

<sup>90</sup> *PSZ II*, vol. 36, no. 36,659, article 64; no. 36,662, article 159; no. 36,663, article 164; no. 36,664, article 138; *PIM*, vol. 1, no. 16 (May 20, 1859) (General Session of the Editing Commission); vol. 3, pp. 16; and vol. 11, p. 136.

<sup>91</sup> *PIM*, vol. 3, p. 19.

<sup>92</sup> *PIM*, vol. 11, pp. 150–52; Kovan'ko, *Reforma*, 148.

<sup>93</sup> *PSZ II*, vol. 36, no. 36,659, article 65; *PIM*, vol. 11, p. 152.

The results proved devastating. After emancipation, many nobles, though willing to have their ex-serfs acquire land in excess of the maximum, refused to do so without a government guarantee for the proper receipt of payment. Landlords feared that, without seigneurial authority, it would be difficult to compel peasants to fulfill private obligations. To many peasants, the only difference between a voluntary agreement and one demanded by the landlord was that the voluntary agreement might cost more. Consequently, outside Russia's western provinces, only one in three redemption settlements was reached on the basis of voluntary agreement.<sup>94</sup>

The government went further to minimize the amount of redemption paper that would have to be put in circulation and then to delay its issue. Beyond the restrictions limiting the size of a redemption grant and the exclusion of peasants on *corvée* from the redemption process, the law required that all outstanding mortgage debt to the state credit institutions be deducted from the total sum due the landlord (Article 69). Only the balance was to be issued in credit paper. The Finance Commission maintained that "this substantially facilitates the redemption operation . . . All that is required from the government is a guarantee of the debt to the credit institutions without the issue of new credit paper, which is the greatest of all difficulties in the present matter."<sup>95</sup> Serfowners were not given the option of receiving the full redemption valuation of their land and then continuing to pay off existing mortgage debt. Thus redemption was a massive, even if voluntary, foreclosure. To use official terminology, bank debts of nobles "were transferred" to the redemption operation. Peasant payments went directly to liquidate bank debt. Once redemption began, landlords lost the "right" to default on their loans, as was their habit on occasion, eliminating yet another risk to state finances. P. Kovan'ko argued that if it had not been possible to retire serfowner debt in this manner, the Finance Commission would have been inclined to propose an even more gradual redemption process. Instead, less credit paper had to be issued, landlords were freed of mortgage debt, state finances were more secure, and the peasants became property owners sooner.<sup>96</sup>

This so-called transfer of bank debts to the redemption operation had yet another dimension of fiscal caution. In 1857, after interest rates on loans had been reduced, serfowners paid 4 percent interest. To amortize the debt, they paid an additional 2 percent for twenty-eight years or 1.5 percent for thirty-three years. When bank debts were transferred to the redemption operation, the government received from the peasants 6 percent (interest and principal) for forty-nine years. What did the Finance Commission propose to do with sixteen or twenty-one years of profit? "The difference ought to revert for the use of the redemption operation itself, increasing its reserve capital, and along with this guaranteeing and

<sup>94</sup> PSZ II, vol. 36, no. 36,659, article 68. See P. Kovan'ko, "Osvobozhdenie krest'ian i obiazatel'nyi vykup: Na osnovanii arkhivnogo materiala," *Russkaia Mysl'* (June 1912): 27; Vil'son, "Vykupnye za zemli platezhi," 278–80. If a landlord demanded redemption, the law prohibited any supplemental peasant payments. If redemption was voluntary, beyond the maximum redemption grant, the landlord and peasants were free to reach any agreement on additional payments.

<sup>95</sup> PIM, vol. 3, p. 22. See also PSZ II, vol. 36, no. 36,659, article 143; Kovan'ko, *Reforma*, 299.

<sup>96</sup> Kovan'ko, *Reforma*, 288. It should be noted that while the government was a lenient lender, on the whole serfowners did pay their bank debts, though not always on time and often only under threat of foreclosure.

accelerating the proper conclusion of redemption."<sup>97</sup> No thought was given to reducing the size of peasant payments from the outset in anticipation of these revenues. Again, the Finance Commission exercised extreme, if not excessive, caution in ensuring the solvency of the redemption operation and in protecting the state treasury.<sup>98</sup>

Of the 948 million rubles of serfowner land to be allotted to the peasants, the commission estimated that the government would accept in mortgage a maximum of 758.5 million rubles, 80 percent of its total capitalized quitrent value. From this, 425 million rubles of landowner debt would be deducted, leaving 333.5 million rubles to be issued in credit paper.<sup>99</sup> Because redemption was limited to peasants on quitrent, the commission thought it unlikely that more than 2.5 million peasants would begin redeeming their land in the first five years, thereby requiring no more than 50 million rubles in credit paper to be issued during this period.<sup>100</sup> And the redemption paper issued was to be restricted in its circulation in order to protect the credit markets.

The law intended that, once redemption began on an estate, the noble landowner, though left with less land than before emancipation, would still possess substantial holdings, be free of debt, and hold two kinds of bonds, some readily convertible into cash and others yielding an annual income. The convertible bonds, coupled with new forms of long-term mortgage credit then being proposed, would give landlords the capital necessary to restructure their estates along commercial lines. If the redemption agreement was a voluntary one, the serfowner could also receive cash payments directly from the peasants.<sup>101</sup>

The intent of the Finance Commission was to keep much of the credit paper issued for redemption off the market. Under the law, most serfowners were to receive both 5 percent bank notes and redemption certificates. Only redemption grants of under 1,000 rubles were to be in bank notes alone. For progressively larger amounts, only one-fifth to one-twentieth was to be in bank notes, with the remainder in certificates (Article 70). Second series (redemption) bank notes or bonds were not to be issued in amounts of less than 50 rubles, to prevent them circulating like cash (Article 146).<sup>102</sup> But they could be freely traded on the bond market and, therefore, readily converted into cash.

In contrast, redemption certificates were not to be issued in denominations of less than 3,000 rubles when possible and under no circumstances in amounts of less than 300 rubles (Articles 144 and 145). Redemption certificates could not be traded on any exchange. Rather, they had to be issued in a person's name and could be transferred only in accordance with procedures governing immovable property, that is, by deed of purchase (Article 147). Large denominations and inconvenient exchange procedures, the commission reasoned, were necessary "for the soundness of internal credit, since the simultaneous issue of a huge

<sup>97</sup> *PIM*, vol. 11, p. 194.

<sup>98</sup> Kovan'ko, *Reforma*, 9, 288–89, 299.

<sup>99</sup> *PIM*, vol. 11, p. 172.

<sup>100</sup> *PIM*, vol. 11, pp. 173–74.

<sup>101</sup> Lamanskii, "Iz vospominanii," 3: 588; N. Kh. Bunge, "Kredit i krepostnoe pravo," *Russkii Vestnik*, 4 (1859), part 2, pp. 336–39; Kovan'ko, *Reforma*, 150, 313; Migulin, *Vyкупnye platezhi*, 3–5.

<sup>102</sup> *PIM*, vol. 11, p. 205.

number of new obligations could have an unfavorable influence on their rate of exchange and consequently [they] could circulate to the disadvantage of the holders themselves and interfere with the successful development of the entire operation."<sup>103</sup> As the *Stock Exchange News* explained to its readers, "Everybody understands that the basis of the redemption operation by means of 5 percent certificates, the exchange of which has to be undertaken by deed of purchase, all but coincided with the transformation of our credit institutions, most especially with the consolidation of bank debt by means of the issue of 5 percent [first series] bank notes. The fear of suddenly flooding the money market seemed at the time quite natural."<sup>104</sup>

The Finance Commission worried that 5 percent government-guaranteed redemption paper, flooding the market and selling substantially below nominal value, would compete with other investment options. Such a flow of free capital into redemption paper would hamper railroad development, adversely affect government borrowing, and push interest rates up even further. Thus, although the commission often spoke of the benefits of providing ex-serfowners with a long-term, guaranteed income, its intent was to keep redemption paper off the market.

IN SUM, PEASANT REFORM was not accorded a high financial priority. Unlike banking reform, the government was not to subsidize peasant acquisition of land. Nor were state or transport credit needs to be hampered by the redemption operation. The law was designed to keep total redemption debt within strict limits. The restriction of a redemption loan to 80 percent of the collateral's value and the transference of bank debts onto the peasants greatly limited the amount of credit paper that would have to be issued. Coupled with the high interest rates charged to the peasants, the transfer of debts provided for the accumulation of a substantial capital reserve to cover all redemption costs and contingencies. To protect credit markets from the sudden issue of paper so soon after the banking crisis, redemption was to be voluntary; in order to be gradual, it was to exclude peasants on *corvée*. For the same reason, serfowners were not to be given liquid assets in compensation. Redemption was to stand alone and apart.<sup>105</sup>

Not all worked out as planned. On June 27, 1862, as a result of serfowner pressure and peasant discontent, *corvée* estates were made eligible for redemption.<sup>106</sup> Between March and November 1863, immediate mandatory redemption was ordered in the western provinces to prevent the spread of a revolt of the Polish nobility to the peasantry.<sup>107</sup> On June 24, 1863, the government rescinded the requirement that redemption certificates be exchanged by deed of purchase and permitted their transfer by notaries and stockbrokers. Although the certifi-

<sup>103</sup> *PIM*, vol. 3, p. 23. See also Bunge, "Kredit i krepostnoe pravo," 367; Lamanskii, "Iz vospominanii," 3: 587–88; Kovan'ko, *Reforma*, 61; Migulin, *Vykupnye platezhi*, 2–3.

<sup>104</sup> *Birzhevye Vedomosti*, 274 (December 22, 1862).

<sup>105</sup> Migulin, *Vykupnye platezhi*, 7–8.

<sup>106</sup> *PSZ II*, vol. 37, no. 38,407.

<sup>107</sup> *PSZ II*, vol. 38, no. 39,337 (March 1, 1863); no. 39,928 (July 30, 1863); no. 40,172 (November 2, 1863).

cates still carried more restrictions than bank notes (bonds), the market viewed them with more favor.<sup>108</sup> These sudden revampings of the redemption law reveal a lack of foresight among the autocracy's financial experts in regard to the broad social and political dilemmas in society.

In all, instead of a maximum of 2.5 million male peasants beginning to redeem their allotments in the first five years, as the Finance Commission expected, 3.6 million males concluded redemption transactions. The total value of these redemption grants was 329.5 million rubles, of which 48 percent was withheld for bank debts. By January 1, 1867, a total of 179 million rubles in redemption paper had been issued.<sup>109</sup>

Despite the law, the market won out over legal restrictions. Between 1862 and 1866, the easily exchanged bank notes kept their value, while restricted paper traded at a deep discount. As early as May 1863, bank notes could be realized for 97 to 98 percent of their value. By contrast, redemption certificates sold for only 80 percent of face value and the 5.5 percent annuities issued for the redemption of corvée estates only 78 percent, though they paid higher interest.<sup>110</sup> In order to ensure that redemption did not entail such large losses for serfowners that they would refuse to enter into any redemption agreements and to maintain the value of other credit paper, the government was forced to ease the restrictions on the exchange of certificates and annuities. This improved their value considerably. Even if certificates and annuities never traded as freely as bank notes, by mid-1864 the gap in market value closed roughly ten points, half of what it had been before.

But the overall picture remained bleak. The banking crisis had forced the issuance of more than 420 million rubles in long-term bonds and perpetuities. The *Stockholders' Journal* noted that many companies in their initial stock offerings of 1857 and 1858 had counted on capital in the state credit institutions being invested in their enterprises. "These hopes were not realized. Bank deposits were converted primarily into new 5 percent notes, and then our vital financial resources ran out."<sup>111</sup> Consequently, the stock market suffered a sharp reversal, and a period of tight money was ushered in. Bunge and Lamanskii continued to defend debt conversion as having restructured capital long since wasted by the government and serfowners. They attributed the continuation of the financial crisis to an excessive demand for capital. As early as September 1860, Bunge stated that "whereas at first the crisis was attributed to a surplus of paper, now, with tight money prevailing," many were blaming the conversion of bank deposits.<sup>112</sup> Lamanskii argued persuasively that the huge issue of paper money during the Crimean War had temporarily masked the true shortage of capital

<sup>108</sup> PSZ II, vol. 38, no. 39,779 (June 24, 1863). See also *Birzhevye Vedomosti* (July 13, 1863).

<sup>109</sup> *Otchet gosudarstvennogo banka*, 67. See also N. Kh. Vessel', *Nasha novaia gosudarstvennaia finansovaia deiatel'nost' (1892–1894 gg.)* (St. Petersburg, 1894), 10.

<sup>110</sup> *Birzhevye Vedomosti* began regularly quoting the market value of all redemption paper in May 1863. See also *Birzhevye Vedomosti*, 274 (December 22, 1862); *Ezhegodnik ministerstva finansov za 1869* (St. Petersburg, 1869), 163–81; Kovan'ko, *Reforma*, 61–62, 311; Migulin, *Vykupnye pliatezhi*, 8–9; Bliokh, *Finansy Rossii*, 2: 52, 57, 77.

<sup>111</sup> *Zhurnal dlia Aktsionerov*, 172 (April 27, 1860).

<sup>112</sup> N. Kh. Bunge, "Zametki o sovremennom ekonomicheskom krizise," *Zhurnal dlia Aktsionerov*, 194 (September 28, 1860).



from which Russia suffered.<sup>113</sup> On the eve of emancipation, the *Economic Indicator* reported, "At present, with tight money common, with the decline of joint-stock companies and enterprises of various sorts, in a word, with a general financial-commercial crisis, it is scarcely possible to meet two people who do not mention the word credit in the course of conversation."<sup>114</sup>

In England and France, only a small portion of capital was tied up in joint-stock ventures when the financial crisis hit in 1857. But in Russia's malnourished capital market, the adoption of a similar innovation proved disastrous. Wide application of new methods of finance in an unprepared environment put the entire modern sector of the economy at risk. Numerous transport firms went bankrupt, and others were forced to scale back. Faith in private ventures through joint-stock companies was shaken for years. By the end of the 1860s, this attempt at borrowing foreign methods of private finance was openly criticized.<sup>115</sup> The fact that the banking crisis in Russia came on the heels of a European financial crisis illustrates a disadvantage of being dependent on the savings rates of other countries. When Russia's traditional sources of foreign capital dried up, attempts to raise funds internally proved ruinous. But it is not surprising that the machinery of Western finance capital failed in Russia of the 1850s nor unusual for financial experimentation to precipitate crisis.<sup>116</sup>

Once the terms of redemption became known, and then were extended, the credit required for redemption deepened the crisis and prolonged the period of tight money. The promise of new forms of long-term mortgage credit for noble landowners could not be kept. The 1859 Commission on Land Banks, headed by Hagemeister, had recommended the establishment of private institutions financed by landowner capital. The commission, in a sharp reversal of longstanding government policy, opposed the expenditure of state funds to finance agricultural credit.<sup>117</sup> Given the tight market, it proved impossible to raise funds privately. Thus the agricultural system foreseen by the reformers—hired wage labor using modern landowner inventory—was not realized. Rather, it yielded to other forms of tenancy, especially small-scale sharecropping or land rental agreements, which required little capital outlay by the landlord. Certainly, the financial crisis hampered noble landowners inclined toward agricultural improvement—few though they were.

All in all, the redemption operation and the harsh terms imposed on the peasants were to a considerable extent a response to fiscal constraints dictated by the collapse of state credit institutions. It was the autocracy's ill-fated decision to rely on undeveloped private capital markets to finance transport development that hindered growth, pushed interest rates up, lowered the value of redemption paper, and burdened the peasantry for decades. But the failure of the policy facilitated a return to Russian autocratic traditions of substantial state involve-

<sup>113</sup> *Birzhevye Vedomosti*, 43 (February 21, 1861).

<sup>114</sup> *Ukazatel' ekonomicheskii*, 17 (February 26, 1861).

<sup>115</sup> *Zhurnal dlia Aktsionerov*, 196, 199, 201 (October 12, November 2 and 16, 1860); *Narodnoe Bogatstvo*, 6 (November 7, 1862); *Birzhevye Vedomosti*, 81, 138 (March 27 and May 28, 1868).

<sup>116</sup> Hoppit, "Financial Crises," 50.

<sup>117</sup> *Trudy komissii ustroistva zemskikh bankov*, 2 vols. (St. Petersburg, 1860), 1: vii–x; *Narodnoe Bogatstvo*, 74 (April 7, 1863).

ment in society. Still, state intervention in the economy in the early stages of industrialization under conditions of extreme backwardness, subsequently conceptualized by Gerschenkron, required the political will to be innovative and to search for alternatives. There was a need to formulate a distinctly Russian industrial policy rather than merely transplant foreign methods of finance, to recognize broader social and economic constraints rather than simply address fiscal limitations. Only then would politics be in concert with economic necessity. Yet it was precisely this vision that these reformers lacked.

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## Reviews of Books

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### GENERAL

WILLIAM H. MCNEILL. *Arnold J. Toynbee: A Life*. New York: Oxford University Press. 1989. Pp. viii, 346. \$24.95.

It is fitting that William H. McNeill, who has gone intrepidly where few historians are willing to follow, should write a biography of Arnold Toynbee. McNeill has produced a sympathetic and critical account of Toynbee's study of the past. Faced with the biographer's dilemma of the relative weights to assign to the "life" or the "times," McNeill's psychological and scholarly life of Toynbee explains the thinking and conduct of the mature man as a consistent development of his early emotional and intellectual experience. The most important of those experiences, McNeill argues, were his attempts to fulfill his family's great expectations and his own remarkable record at school; his avoidance of military service in World War I, which led him to attempt to vindicate his office job by contributing to a permanent peace; his irrational fear of poverty; his mystical religious experiences at times of great personal crisis; and his habitual immersion in solitary desk work as a way of escaping personal problems. Toynbee's constant and prodigious research and writing in world and ancient history, classics, government reports, and journalism were to blame, McNeill contends, for the failure of his marriage to Rosalind Murray, the daughter of the classicist Gilbert Murray, and for his eldest son's suicide.

Relying essentially on Toynbee's correspondence and publications, McNeill follows his career from a Balliol first in Literae Humaniores, or "Greats," in 1911, through a tour of classical sites in Italy and Greece that indelibly marked his later thinking, to a Balliol fellowship and the teaching of ancient history in 1912, which he abandoned after the war. In the newly established Political Intelligence Department, adjunct to the Foreign Office and staffed by academics, Toynbee became responsible in 1917 for political intelligence on the Ottoman empire and then on Central Asian Muslims. He tried and failed to persuade David Lloyd George to make a Turkish peace that would prevent conflict between Islamic peoples and the British empire. In 1919, he became the first

Korae Professor of Greek and Byzantine History, Language, and Literature established at King's College, London, by nationalistic Greeks and philhellenes. Toynbee was compelled to resign in 1923 after attacking Greek atrocities in the Greek-Turkish conflict. Although accepting his resignation, the university learned to beware of Greeks and other nationalities bearing gifts, and they modified the endowment to protect the independence of future holders of the chair. A temporary one-year post in 1924 turned into a thirty-year position with the Royal Institute of International Affairs at Chatham House, funded largely by the Bank of England and John D. Rockefeller. Toynbee, with the considerable help of the modest Veronica Boulter, whom he married after Rosalind left him, produced yearly books surveying contemporary international affairs. If Toynbee had read modern history at Oxford, he might never have attempted such wide-ranging syntheses of recent historical events. After an inaugural lecture in 1926, in the Stevenson Chair created for Toynbee at the University of London, he never lectured again and in 1928 became Research Professor of International History. Toynbee traveled extensively during his career, and McNeill argues that those travels, especially to non-Western countries, confirmed his pre-1920 view that civilizations, essentially separate and unable to communicate significantly, went through the same cycle of rise and fall.

Part of the popular success of *A Study of History*, McNeill demonstrates, was a result of the coincidence between Toynbee's views and contemporary historical events from the mid-1930s to the 1950s. The first volumes, emphasizing war as the cause of the disintegration of civilization, appeared in 1934, a year after Adolf Hitler's rise to power, a year before France and Britain largely ignored the Italian invasion of Ethiopia, and two years before the Axis alliance. In the United States, that early success was promoted by the Henry Luce publications, which made Toynbee a media star. But the last volumes, published in 1954, despaired of Western civilization to argue that spiritual progress, a product of human suffering, imbued history with its meaning. McNeill emphasizes that Toynbee's political, intellectual, and private thought and behavior were riddled by con-

traditions. But he claims for him an innovative extension of historical sympathy and interest beyond traditional Western subjects. And, instead of concentrating on Toynbee's inaccuracies, McNeill pleads that he should be judged, as artists are, by the effect of poignant personal and public experiences on his work. But, as McNeill so effectively portrays him, Toynbee was monumentally selfish, irresponsible, and mean in every sense of the term. What reputation remains to him as a pioneering historian of the world rests on what he wrote in historical time.

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LUTZ NIETHAMMER. *Posthistoire: Ist die Geschichte zu Ende?* Assisted by DIRK VAN LAAK. (Rowohlt's Enzyklopädie, number 504.) Reinbek: Rowohlt Taschenbuch. 1989. Pp. 189. DM 16.80.

Lutz Niethammer is undoubtedly the leading practitioner of oral history in Germany, known not only for his earlier studies of the miners in the Ruhr Valley in the period of industrialization but more recently for the two major interview projects that probed into memories of the Nazi *Alltag* among working men and women in the Ruhr and in the German Democratic Republic (GDR)—the first time that a Western team was permitted to carry on such a study in the GDR. This essay combines a historical analysis of the idea of *posthistoire*, the notion of the destruction of meaning in a technological mass civilization, with a critical examination of the supposition, so fashionable since Nietzsche, that history has in fact come to an end. Niethammer places the idea of *posthistoire* in its political context; he sees it as a response by an intellectual elite to the emergence of a mass culture. Until World War II, the idea of *posthistoire* was associated with a political and cultural Right. Niethammer traces this line of thought from Nietzsche and Spengler to the protofascist thinkers of the interwar and war periods in Germany and France: Martin Heidegger, Hendrik de Man, Ernst Jünger, Carl Schmitt, Jacques Doriot, Hans Freyer, Paul de Man, and Arnold Gehlen. Hostile to technology, Jünger and those like him were fascinated by technology as the portent of a dangerous life in which violence and death could give new meaning to existence in the midst of cultural petrification. In the wake of Nazism and disillusion with the Soviet experiment, thinkers who viewed themselves as coming from Marxism, such as Walter Benjamin and Max Horkheimer, were won over to the notion that the Enlightenment belief in rationality, which they now identified with technical rationality, had destroyed meaning and culture. Nietzsche and Heidegger soon paradoxically became the culture heroes of thinkers on the Left who sought emancipation from the logic of domination and manipulation,

which they believed were inherent in science and technology.

In the final critical part, Niethammer suggests that the conception of *posthistoire* rests on a mistaken metaphysical assumption. In repudiating Marx and Hegel, advocates of *posthistoire* accepted the idea, which Marx and Hegel had unwittingly inherited from Judeo-Christian eschatology, that history requires fulfillment. Proceeding from an elitist conception of culture, the theorists of *posthistoire* in essence mourned the passing of the *Bürgertum*, which they had identified with history (p. 15). From Nietzsche and Heidegger to Benjamin and Adorno, they have been "mesmerized" by problems of the past, "by symptoms of the rigidification of civilization which they exaggerated instead of giving attention to the capacity of self-destruction" confronting the present world (pp. 164–65). Their conception of an anonymous mass amounts, for Niethammer, to an empty and mistaken construction that submerges the real individual human beings who compose history in "the fiction of an objectively meaningful process of history" (p. 171). In the place of the megahistorical conception on which the notion of the end of history rests, he calls for a much more humble "history from below" (p. 170), which endows history with a human face. Niethammer's essay constitutes a thoughtful corrective to the commonplaces so uncritically shared by a broad segment of modern scholars of historical thought.

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ROBERT BONNAUD. *Le Système de l'histoire*. Paris: Fayard. 1989. Pp. 336. 140 fr.

Historians worried by the atomization of their profession and the fragmented picture of the world that decades of specialized monographs and descriptive histories have produced should welcome this lively and imaginative book. Armed with an arsenal of novel concepts and a zest for synthesis, Robert Bonnaud challenges many of the presuppositions and practices of modern historical research and proposes a compelling version of universal history.

By universal history Bonnaud means not (or not only) the empirical history of human evolution, which, he claims, can never generate a unitary science of humanity, because it can never identify the fundamental unity of the human world within its obvious and divisive diversity. Empirical history is further restricted by its canons of research based on chronology, causality, influence, individuality, documentability, and "Occidentalism" (chap. 15). Universal history, for Bonnaud, must build on noology, the "science" of the "noosphere" (a term popularized in the 1950s by, among others, Catholic theologian and paleontologist Pierre Teilhard de Chardin), which refers to the

specifically human realm of thought, action, and activity and which is present always and everywhere throughout human evolution. Noology, he contends, produces a universalizing vision and calls for a "planetary eye" (p. 37).

Building on this vision, Bonnaud challenges the conventional and limiting notions of the unidirectionality of history and the singularity of each historical epoch in favor of a schema of historical periodization organized into stages, phases, and acts that are discernible in all spheres of human activity and in all epochs and that allow historians to chart historical change and the geography of the human spirit. This schema calls for thinking in terms of alternations and repetitions, symmetrical and asymmetrical structures, horizontal and vertical relationships, analogies and homologies, rhythms and fluctuations, contractions and expansions, and recognition that the "progress" of the human spirit is nonlinear and proceeds in zigzag fashion. Bonnaud moves with graceful authority among the disciplines, between prehistory and contemporary history, between East and West, and between history as fact and history as process, and he brings such diverse thinkers as de Chardin, Arnold Toynbee, Karl Marx, Gaston Bachelard, and a host of historians and social scientists of every period and persuasion into novel and fruitful discourse.

In Bonnaud's "system of history," epistemological positions are inseparable from moral and political positions. History is never neutral; it always structures the phenomena it studies. It is only a question of which perspectives historians should encourage and which they should not. Bonnaud's own agenda is unequivocal: historians should be for universality and against particularism of every kind, whether nationalistic, ethnocentric, or class privileging. He rejects those who smugly proclaim that there is no history but only histories in the plural and who conceive of "l'histoire-connaissances" as a "federation of ghettos" (pp. 222–23).

Bonnaud tries hard to do justice to the diversity and unevenness within the unity of the human world he posits. He readily concedes, for example, that Europe and the West have played a preponderant role in world affairs over approximately the last three centuries, which he explains by "the law of unequal development" (chap. 18). Nevertheless, his elaborate conceptual apparatus and abstract terminology (explained in a glossary) tend at times to obscure the "otherness" of the Other and to conjure up the McLuhanesque image of the coming world as a "global village." Not all readers will relish that prospect. But this book should appeal to historians eager to venture beyond their fields of specialization to explore and expand the limits of historical knowledge.

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ALAN F. SEGAL. *Paul the Convert: The Apostolate and Apostasy of Saul the Pharisee*. New Haven: Yale University Press. 1990. Pp. xvi, 368. \$29.95.

Alan F. Segal writes avowedly as "a believing Jew and a twentieth-century humanist" (p. 281) and as such undertakes a remarkably sympathetic assessment of Paul. In calling him "Paul the convert," Segal defines conversion as a change in religious community. There were other Pharisees among the followers of Jesus in the first Christian generation, but following Jesus did not constitute a conversion. The Pharisees added acknowledgment of him as Messiah to their existing pattern of belief and practice but did not abandon that pattern; they preserved their Pharisaic devotion to the Law, as Paul did not.

Segal is probably right in understanding Paul's conversion experience in terms of *merkabah* mysticism, the goal of which was the reproduction of Ezekiel's vision of the glory of God, seen as "a likeness as it were of a human form" above the divine chariot-throne. The distinctive feature of Paul's experience was that he identified the human form with the exalted Jesus: indeed, in Luke's narrative the vision is accompanied by a voice—one might almost say a *bath qôl*—that says explicitly "I am Jesus." Paul's Christology and many other aspects of his gospel can be traced back to this conversion experience.

But how did his conversion constitute a change in religious community? Certainly, the displacement of the Law as the center of his life and thought meant that he could no longer belong to the fellowship of Pharisaic Judaism. According to Segal, "Paul converted into a vibrant and dynamic albeit small gentile church, which provided him with a social context in which to continue education" (p. 267). But gentile evangelization had barely begun at the time of Paul's conversion, and there is no suggestion that it had affected the disciples of Damascus, among whom he first found Christian fellowship. By the time he moved to Antioch, the headquarters of gentile Christianity, he had himself been engaged for some years in the evangelization of gentiles, beginning with the Nabataean Arabs. The implication of his own testimony (Gal. 1:16) is that the commission to evangelize gentiles was part of his conversion experience. In forming his gentile converts into local communities, along with Jewish followers of Jesus, he did not treat them like the God-fearing gentile adherents of the synagogue but incorporated them as full members of the people of God, without the requirement of circumcision or any of the other features of the Law that demarcated Jews from gentiles. If those features were not necessary for gentiles, then logically they were not necessary for Jews either, and in this tendency of his teaching to lead people astray lay his "apostasy," according to Segal (p. 223).

Segal devotes a chapter to the "Noahide Laws" (pp. 187–223), the injunctions laid on Noah and his descendants (Gen. 9:1–17), which Jewish exegetes



treated as an ethical code for gentiles. Of these injunctions, reckoned to be seven in number, six were already in force as creation ordinances, and these Paul accepted for himself and his gentile converts. On the seventh, the ban on eating meat with blood still in it, he said nothing, but because he permitted his converts to accept dinner invitations from pagan friends, if they felt so inclined, and to eat whatever was set before them without question (1 Cor. 10:27), he plainly attached little importance to the injunction.

Here Paul differed from the Jerusalem church, which, while waiving the circumcision requirement for gentile Christians, maintained the ban on eating blood together with that on eating the flesh of animals sacrificed in pagan temples (Acts 15:29).

Segal's work abounds in welcome fresh insights for students of Paul, including his sociological exegesis of debatable texts. For example, he describes Rom. 7:7–25, often treated as the experience of a divided heart, as “the stuff of tragedy”—Paul's “own reflection on his attempt to make a single community by accommodation in ritual but not in principle” (p. 253).

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JEREMY COHEN. *“Be Fertile and Increase, Fill the Earth and Master It”: The Ancient and Medieval Career of a Biblical Text*. Ithaca: Cornell University Press. 1989. Pp. xiv, 375. \$44.95.

Jeremy Cohen's study of a single biblical verse, Gen. 1:28, is a brilliant piece of intellectual history. This verse, which in abbreviated form is the book's title, has in our time become the concern of many environmentalists because of its apparent conferral of a license to exploit the earth. The comprehensive analysis given here of the use and interpretation of this “primordial blessing” by scholars, teachers, and other authors in both the Jewish and the Christian traditions clearly demonstrates, however, that the verse was not so understood in earlier centuries. Rather, from before the Common Era through the later Middle Ages, it was “appreciated . . . as a statement of anthropology sooner than ecology” (p. 310). Much more attention was also given to the first half of the verse, that is, to peopling the planet rather than to dominion over it.

Another aspect of central importance in the understanding of this verse was its covenantal significance both for the priestly document in the Pentateuch in which it appears and for most of the subsequent intertestamental, rabbinic, patristic, and scholastic commentators on this text. The verse helped spell out the relationship between God and his human creatures, or, as Cohen puts it, it was important for “defining a cosmic frontier” (title of chapter 2). Many writers found here a tension to be resolved between the belief that the first humans were created in the image of God with the potential of immortality and

the inescapable fact that human beings reproduced sexually like animals, some of whom were themselves also commanded to “be fruitful and multiply” (Gen. 1:22 RSV). Christian teachers were especially troubled by the lust that accompanied human sexuality. Nonetheless, Augustine, the most influential of them all, concluded that sex was not inherently evil and that the first couple would have had offspring in paradise “without the tumultuous ardor of passion” (as cited by Cohen, p. 250) had they remained obedient to God.

Drawing on the mainstream of modern historical and literary biblical criticism, Cohen examines the text within its biblical setting, including the creation narrative of Gen. 1–2, the aftermath of the flood in Gen. 9 where the blessing of fertility is renewed for Noah and his sons, and the covenant with Abraham in Gen. 17, and Cohen shows how the text becomes “a formularized guarantee of divine protection” (p. 33).

In separate chapters he presents the rabbinic theology of the Aggadah and the rabbinic legal tradition of the Halakhah. Important themes in the first are sexuality and the natural order, power and dominion, divine blessing and grace, promoting the image of God, and preparing for the Messiah. In the second, he explains how the blessing came to be understood as a command, the obligation to procreate, and to whom it applied, Jewish men. The implementation of this command is dealt with in a chapter on the medieval rabbinic response and biblical commentaries. This chapter also explores the place of this text in the literature of the Kabbalah with emphasis on the *Zohar*.

A parallel chapter on Christian interpretation in the medieval period treats canon law, religious poetry, and the scholastic theologians such as Thomas Aquinas. It also looks briefly at Jean de Meun and Chaucer. Cohen suggests possible common cultural and social influences on the two religious communities for whom Gen. 1:28 was so important. “Perhaps it is not mere coincidence that the *Roman de la Rose* and the *Zohar*, both works of the thirteenth century, accord much significance to the biblical mandate for procreation in a man's quest for fulfillment, and they portray the goal of that quest in the erotic symbol of the rose” (p. 312).

The wealth of documentation and the wide range of materials covered by this book make it a model of contemporary scholarship and an excellent starting point for a variety of related studies. I am persuaded that Cohen has established his conclusion about the primary meaning of this text: “an assurance of divine commitment and election, and a corresponding challenge to overcome the ostensive contradiction between the terrestrial and the heavenly inherent in every human being” (p. 313).

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SAMUEL KINSER. *Rabelais's Carnival: Text, Context, Metatext*. (The New Historicism: Studies in Cultural Poetics, number 10.) Berkeley and Los Angeles: University of California Press. 1990. Pp. xii, 293. \$35.00.

Samuel Kinser's book demonstrates how "metatexts," interpretive studies of Rabelais during the past three hundred years, have obscured and distorted his text. Metatexts, which Kinser says begin as visions of the text but subsequently become images, then plaster molds or icons, present misinterpretations of the relationship between Rabelais's text and his context, or, in more recent semiotic studies, metatexts have viewed his text only as the product of other texts.

Seeking both to expose the errors in metatexts and to find a surer analytic method of getting at Rabelais's meaning, especially that of the Carnival or Lent episode in the *Fourth Book*, Kinser emphasizes the importance of "paratexts," by which he means the various subsidiary but crucial paraphernalia of a text such as the title page, dedication, table of contents, prologue, preface, illustrations, footnotes, and format. Paratexts reveal how texts are related to contexts, how authors attempt to control readers' interpretations of their writings, and how authors draw readers—in Rabelais's case both a popular and an elite audience—into their texts, almost as if they were communicating orally. Insisting that paratextual analysis can reveal how metatexts have wrongly interpreted Rabelais's text as carnivalesque, Kinser emphasizes that Rabelais deliberately used juxtapositions of humor and theology, medicine and politics, and sexuality and scatology to foil or ridicule the criticisms made of him by the Parlement of Paris and other authorities and to elicit favorable reactions of his works. Rabelais's deliberately confusing, often-contradictory way of treating Quaresmeprenant (Carnival), the Sausages, Pantagruel, and other characters was a means to snare his audience by puzzling them. Plot, characters, symbols, and metaphors are stitched together in apparent incoherence. But the psychic ambivalence and social inversions inherent in Rabelais's depiction of Quaresmeprenant enabled him to link the fantastic characters to the readers' real context of Carnival.

Why was Rabelais's meaning so badly distorted in the metatexts? Kinser explains that, in the period of the wars of religion when Rabelais was read widely, his texts were altered so that they could be used by both Catholics and Protestants. After 1610, when there was a marked decline in editions of his work, Rabelais was viewed as being too irreligious. More important, his cultural context had disappeared, and he lost his popular readership. The taste for Rabelaisian humor waned. The elite participated less in Carnival with the result that the great significance of Carnival for Rabelais's text was no longer understood. The metatexts with their different contexts, however, had a life and evolution of their own. They

oriented subsequent editions and evaluations of Rabelais. Kinser identifies three broad categories of metatexts: those that interpret the text by means of ideas or ideologies outside the text; those that explain the text by means of linguistic, semiological analysis; and those that use the historical context and the author's biography to penetrate the text's meaning.

Kinser's book concludes with a critique and an appreciation of Mikhail Bakhtin's carnivalesque interpretation, which he finds both distorted and important. He denies that the festivity and mockery of official life that Bakhtin found existed in Rabelais's text or in Renaissance Carnival. Kinser favors stressing Rabelais's communalism, which he argues was nonexclusive, not related to one undifferentiated populace, not suggestive of later popular and elite distinctions. Yet he lauds Bakhtin for understanding the centrality of folkloric and popular themes in Rabelais. Bakhtin realized that Rabelais used image systems with the result that we are now better able to understand the relation of text to context.

The strengths of this book lie in its analytic apparatus, thorough knowledge of Rabelais, and the metatextual interpretations of him. But is Rabelais as convoluted, ambiguous, and mysterious as Kinser makes him appear? One wonders if Kinser's metatext is closer to the real Rabelais than earlier ones. Nonetheless, this is a richly informative book that will doubtless appeal more to literary scholars than to historians. It is not a book on popular culture, but scholars interested in Rabelais, in metatexts, and in linguistic analysis generally will be rewarded by reading it.

ROBERT M. ISHERWOOD  
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MARGARET R. MILES. *Carnal Knowing: Female Nakedness and Religious Meaning in the Christian West*. Boston: Beacon Press. 1989. Pp. xv, 254. \$24.95.

Margaret R. Miles's latest book seeks to account for the ways in which Western artists—overwhelmingly male—have portrayed the unclothed female body in paintings and sculptures that deal (at least overtly) with religious themes. On this topic she has much to say that is interesting and novel.

Representations of naked women in the past, Miles argues, have typically reflected male fears and fantasies, because until relatively recent times self-representations by women were exceedingly uncommon. In search of the rationales that underlay traditional male depictions of unclothed women in Western religious art, Miles ranges through numerous fields of scholarship and seeks to identify commonalities in treatments of the naked body. She examines, among other things, the practice of naked baptism in the fourth century; verbal descriptions of female martyrs and ascetics in patristic literature; contrasting approaches to the portrayal of Adam and Eve, both in

painting between the twelfth and the fifteenth centuries and in literature between the fourth century and the nineteenth; representations of Susanna and the elders; images of witches and of women among the damned souls at the Last Judgment; and grotesque female figures in Romanesque decorative sculptures.

In such a sweeping survey of so many different periods and forms of expression, it is no doubt inevitable that some of what the author says will raise doubts and reservations in the minds of specialists. As a medievalist, for example, I find it distinctly odd that Miles bases her discussion of medieval views of witches and witchcraft primarily on Saint Augustine, writing in the fifth century, and on the *Malleus maleficarum*, written in the fifteenth century (not in the sixteenth century, as she states on page 154). There are more appropriate sources for this topic, and, had she consulted some of them, her conclusions might have been more complex but also more adequate than they are. It is also disconcerting, to say the least, to find that she generalizes about Western interpretations of Adam and Eve from five such disparate writers as Saint Ambrose, Saint Augustine, Saint Hildegard of Bingen, Martin Luther, and Søren Kierkegaard.

That said, it must be added that Miles's observations and reflections on her sources are often acute and original. Her discussion of the social implications of the naked body and the paradoxical contrast between the messages conveyed by naked men and women, for example (pp. 81–82), is uncommonly perceptive, and her analysis of readings of the Genesis creation myths is helpful (pp. 114–16), even though I have reservations about her selection of sources.

Miles's analysis draws heavily on twentieth-century feminist theory for its insights and intellectual framework. In consequence she predictably and repeatedly falls into the anachronistic fallacy of judging past generations of artists by criteria that they were quite unaware of and could not possibly have shared. Although this may be an instructive exercise in order to demonstrate the gulf that separates modern feminist beliefs and assumptions from the values current among earlier generations, it sometimes does rather less than justice to the authors whose work she examines. Her study, in the final analysis, seems designed not so much to analyze past approaches to the female body as to establish a basis for a political agenda, which she outlines in her final chapter. Because she finds that past representations of women's bodies in religious contexts are inappropriate, she seeks at the conclusion of her study to define a program for achieving what she regards as a more adequate treatment of the theme.

JAMES A. BRUNDAGE  
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MARY KILBOURNE MATOSSIAN. *Poisons of the Past: Molds, Epidemics, and History*. New Haven: Yale University Press. 1989. Pp. xiv, 190.

Mary Kilbourne Matossian has long argued that many disease outbreaks of the past were ergotism, although they may have been diagnosed as something else. In this work she has added a second group of mycotoxins, the several species of *Fusarium* that can contaminate all major cereal crops, but the primary focus remains on rye contaminated with ergot and the "package of chemicals" (p. 13) such contamination could have delivered to the consumer. Ergotism in serious cases has two variants: convulsive and gangrenous. Its other symptoms are myriad, ranging from fever and nausea to hallucinations and psychosis. Clearly, given the range of symptoms, practically any disease of the past from malaria to typhoid could have been ergotism in disguise, and Matossian is not hesitant about pointing this out. Moreover, because ergot toxins can influence fertility and can pass through mothers' milk to kill infants, ergotism or its absence may have acted as an important influence in the history of whole populations.

Matossian begins with Russia as a case study, where climate has frequently been conducive to ergot formation. After correlating a number of variables, she hypothesizes that "in Russia, and perhaps in other rye-dependent parts of Europe, epidemics of fungal poisoning served as the main limit on population growth before 1900" (p. 43). Then, in shifting the focus to Western Europe, she suggests that many of the inconsistencies surrounding the career of bubonic plague can be explained in terms of food poisoning. Moreover, she suspects that a shift in dietary habits may have been at the root of the modern rise of population in Europe.

Victims of witchcraft persecution were suspiciously concentrated in rye-producing areas of Europe. The "Great Fear" of 1789 in France, which erupted during the French revolution, coincided with a rye crop seriously infected with ergot, while in North America those bewitched at Salem were suffering from ergotism as were the victims of the throat distemper epidemic of 1735–36 and, in fact, as were many caught up in the "Great Awakening" of 1741.

Despite the extravagance of the various hypotheses (which quickly become claims), one cannot easily dismiss this vigorously argued study. It is, of course, speculative, but the speculations rest on solid research, careful analysis, and compelling correlations. Matossian's major point—that the role of toxins (especially ergot toxins) in human affairs was a much larger one than has been appreciated—is well made.

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JAMES C. RILEY. *Sickness, Recovery, and Death: A History and Forecast of Ill Health*. Iowa City: University of Iowa Press. 1989. Pp. xvi, 295. \$27.50.

The history of health may be the next field of social history to undergo explosive growth. Both specialists in the area and those who are curious about the work underway will find James C. Riley's book informative and challenging.

After briefly summarizing what has been learned in recent decades about long-term trends in mortality rates, Riley turns to the question of how the extent and experience of illness has changed. He searches for answers in a variety of sources, including the records of largely working-class organizations (friendly societies) that provided disability insurance and death benefits to members and their relatives. The evidence for the period between 1600 and 1900 pertains mainly to Great Britain but is supplemented with data from Belgium and Australia. For the twentieth century he relies mainly on U.S. data.

Riley argues against the widespread view that sickness has diminished as life expectancy has increased. His interpretation of the available evidence is based on a combination of the theory that the accumulation of physiological and socioeconomic stress (insults) at early ages influences the rate of chronic diseases and of mortality at middle and late ages with the theory that at early ages the population is "heterogeneous" with respect to risks of dying (which means that those who die at younger ages are, on average, more frail than those who survive diseases). The combination of theories leads him to expect the "risk of falling sick" (of having an episode of disease) to be unaffected by the secular decline in mortality, although he expects the proportion of illnesses that terminate in death to decline sharply, especially at younger ages. The combination also leads him to predict that the duration of illness (the odds of "being sick") will increase as frail people survive to later ages.

Morbidity and mortality schedules constructed from the records of the friendly societies provide the principal evidence used to support Riley's interpretation. These records are an extremely difficult source because they exclude a large, and probably variable, percentage of persons in both the lower and upper portions of the income distribution, because little is known about the standards of the friendly societies in determining that an individual was incapacitated or how these standards may have changed over time and because sickly people who could not afford to keep up their payments withdrew from the societies as they aged.

Riley is aware of these problems but doubts that selection biases are strong enough to affect the trends with which he is concerned. Unfortunately, he does not provide the evidence needed to assess the conjecture. Until the dimensions of the various potential biases, and their changes over time, are probed more vigorously, one cannot adequately assess the possibil-

ity that the biases are in some degree responsible for the novel results. The stability of the ratio of partial disabilities, which were not covered by the friendly societies (whether acute or chronic), to those "total" disabilities that were covered is another issue that needs to be probed. Still another issue is the extent to which conceptions of the degree of disability that justifies abstention from work have changed over time.

It would be premature, however, to dismiss Riley's numerous insights and hypotheses about secular trends in illness. Quite the contrary, his book ought to stimulate research into a wide range of questions about trends in health that he is helping to define. Riley has also demonstrated that the archives contain much more data relevant to the resolution of these issues, at least for the nineteenth century, than many have realized.

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WALTER MOORE. *Schrödinger: Life and Thought*. New York: Cambridge University Press. 1989. Pp. xi, 513. \$39.50.

Walter Moore's book is an outstanding contribution to the biographies published coincidentally with many of the centenaries, in the 1970s and 1980s, of the birth of men who made the waning Habsburg empire and the first Austrian republic a center of contemporary culture: Alban Berg and Arnold Schönberg; Oscar Kokoschka and Egon Schiele; Hugo von Hofmannsthal, Robert von Musil, Rainer Maria Rilke, and Franz Kafka; Ludwig Wittgenstein and the physicist Erwin Schrödinger (1887–1961)—a roster in sad contrast with the region's current barrenness.

Moore's biography describes the life and world of men who revolutionized physics in the 1920s. The impact of Schrödinger and his contemporaries (Louis de Broglie, Paul Dirac, Werner Heisenberg) transcends physics: they demolished the deterministic materialism underlying nineteenth-century philosophy. Although Albert Einstein changed our cosmic perception of the universe, quantum physics revolutionized our view of matter on the atomic scale. In contrast to today's physics, this revolution was accomplished by individual scientists without benefit of extensive experimental facilities.

Moore does not share the concern expressed by Stephen Hawking in his popularizing book *A Brief History of Time* (1988) that every equation cuts readership in half. Moore formulates Schrödinger's contribution in terms of equations comprehensible to readers, for example, to me, a reader who, though ignorant of quantum physics, uses mathematical tools of physics. Those not thus equipped need not be deterred: questions and the impact of answers on our view of humans in the universe are accessible to



general readers. Moore provides a picture not only of the evolution of physics and its impact on philosophy but also of the social background.

The book paints a lively portrait of Schrödinger's multifaceted personality. As a physicist he was among the most eminent. Moore quotes the mathematician Arnold Sommerfeld, who describes the discovery of wave mechanics in 1926: "It was the most astonishing of all the astonishing discoveries of the twentieth century" (p. 2).

A second facet of Schrödinger's work falls in the broader scope of "natural philosophy." In his book *What is Life?* (1944), he introduced a concept that became a cornerstone of molecular biology: the function of the chromosome as a "code-script" determining the characteristics of the organism to which it belongs. This little book influenced both the general public and biologists. Francis Crick, of double-helix fame, describes the book as "peculiarly influential" and as having "attracted people who might not have been interested in biology at all" (p. 404). In particular, Crick's American coworker James Watson, in 1946 an undecided undergraduate, wrote, "From the moment I read *What is Life?* I became polarized towards finding out the secret of the gene" (p. 403). Schrödinger had a deep interest in philosophy. He was a lifelong reader of Spinoza and Schopenhauer and of Vedanta and Buddhist scriptures, which he placed above Western religions.

Finally, Schrödinger was a passionate lover, displaying in his fifties the generous freshness of first love. Women readers will smile at the ego of a man who, engaged in a so far unsuccessful pursuit of a colleague's wife, entered this thought, in French, in his diary: "It has never happened that a woman has slept with me and did not wish, in consequence, to live with me for all her life" (p. 272). These words, read by the woman, had the desired effect. Schrödinger's passions almost cost us his contributions: aged twenty-five, in love, but facing too modest an academic salary to afford marriage, he approached his father, begging him to be allowed to dedicate himself to the lucrative family linoleum business. Schrödinger *père*, wisely, refused.

One blemish in Schrödinger's apolitical life is a craven welcome letter to the Nazis, whom he despised, published in the local press celebrating the Anschluss (1938). His true feelings are revealed by his prompt departure for Dublin, where he spent the next eighteen years. He had the candor to reject the excuse that the letter was written under duress.

In summary, Moore has given us a literate, readable biography accessible to scientists and humanists alike. In this book, Moore spans, like Schrödinger, the chasm between the "two worlds" of contemporary culture deplored by C. P. Snow.

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HELGE KRAGH. *Dirac: A Scientific Biography*. New York: Cambridge University Press. 1990. Pp. x, 389. \$44.50.

Paul Dirac (1902–84) was a theoretical physicist and one of those who established quantum mechanics as the basic theory of subatomic phenomena during the decade 1925–35. His major contribution was a relativistic theory of the electron, which led to the prediction of a positive electron, observed soon afterward in a cloud chamber photograph by Carl Anderson at the California Institute of Technology. The positron was the first example of a world of antimatter in which the elementary particles have charges opposite to those in our usual world. Dirac also laid the foundations for a quantum theory of radiation.

Dirac's native genius in theoretical physics barely survived the psychological trauma inflicted by a despotic father (a schoolmaster in Bristol) who drove an elder son to suicide. Paul survived as an inarticulate genius, socially crippled but still capable as a young man of discovering magical realities in mathematical equations. Helge Kragh has elected to relate the history of Dirac's scientific work while excluding almost all references to his personal life. He has deliberately avoided interviews with Dirac's widow and with anyone else who knew him personally. Thus, an already withdrawn personality has been pushed even farther into obscurity. Kragh also denies that Dirac ever had any serious political views, although he was considered by the McCarthyites to be a danger to the American way of life and was barred for some years from entry into the United States. The files of the Federal Bureau of Investigation for this period would have been fascinating historical material, but Kragh has not ventured into this morass.

The substance of Dirac's work is so abstruse that most of this book will be accessible only to cognoscenti of quantum mechanics. Because there is usually no way to abbreviate Dirac's concise scientific style, the biographer has provided a chronological and thematic guide to his work without trying to explain it. The careful scholarship of the author within his limited purview has nevertheless yielded a book of permanent value for historians of science. The interplay of ideas among the contemporary physicists is especially well displayed. At times, however, Kragh seems to assess the value of a scientific theory by its popularity. Thus, he states that Hermann Weyl's unified geometric theory of electromagnetism and gravity "fell into oblivion" (p. 239), whereas it was being actively but perhaps unsuccessfully pursued by Albert Einstein and Erwin Schrödinger from 1935 to 1950 and is still of interest today.

Dirac had no interest in formal philosophy, but he accepted the "principle of plenitude": whatever is not impossible must exist. This led him to propose the existence of a magnetic monopole, a magnetic pole without its partner of opposite sign. Cosmic-ray physicists are still searching for this entity, which would



have great importance in the theory of elementary particles and in cosmology.

This "scientific biography" should be widely read by physicists. One hopes that it may lead some future Dirac to ponder the relation of mathematics to physics, a problem at the heart of physical theory. Dirac was inspired by an undefined concept of "mathematical beauty," but his mathematics was a conventional kind, designed for eighteenth- and nineteenth-century physics, a continuous world of projectile trajectories and planetary orbits. He was able to construct several beautiful theories with these archaic tools, but ultimately they failed him. We are still waiting for a mathematics that is truly appropriate for the discontinuous yet unified world of quantum mechanics.

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EDITH KURZWEIL. *The Freudians: A Comparative Perspective*. New Haven: Yale University Press. 1989. Pp. xii, 371. \$35.00.

This study is an ambitious survey of a wide range of basic and applied psychoanalytic thinking with a stress on various theoretical schools of the latter half of the twentieth century. The book has an encyclopedic quality and is not for beginners, who would have a hard time assimilating the rapid presentation of often-complex material.

Two themes dominate. The first one is an affirmation of Sigmund Freud's influence on every aspect of twentieth-century Western culture at a time when psychoanalysis is often under attack in the United States. The second is that of the domination of post-World War II psychoanalysis by "Anglo-Saxon" ego psychologists, increasingly challenged by other theoreticians. The thought of mainstream American analysts and Anna Freud, on the one hand, and the approaches of Melanie Klein, Heinz Kohut, Otto Kernberg, and Jacques Lacan, on the other, form a contentious dialogue and, in some cases, the beginnings of a theoretical synthesis. The author, a sociologist, effectively uses the happenings at annual meetings of the International Psychoanalytic Association to make many of her points.

According to the dust jacket, the goal of the book is to demonstrate that "every country unconsciously creates the psychoanalysis it needs." Edith Kurzweil summarizes both major and secondary work in theoretical and applied psychoanalysis in the United States, England, France, Germany, and Austria. She concludes that the United States produced a conservative and "narrow" ego psychology, a medicalized psychoanalysis, and a heightened authoritarian psychoanalytic training. The Americans together with the British became wedded to a "scientific" psychoanalysis. In France and Germany, psychoanalysts have been as concerned with social and cultural issues

as with theoretical developments. There is a long history to these analysts' wider concerns, which extends back to the earliest days of the Vienna Psychoanalytic Society. Kurzweil argues that the split between Freud and Alfred Adler before the First World War was as much over politics as it was a personality conflict or a fight over theory. Personalities play an important role in Kurzweil's survey; for example, the burgeoning of psychoanalysis in France after World War II and its ultimate popularity cannot be separated from the ideas and work of Jacques Lacan. Kurzweil concludes that there is little to say about modern psychoanalysis in Austria.

Although we are promised "a comparative perspective," this never truly emerges. The introductory historical chapters are brief and episodic, and neither they nor the rest of the book actually demonstrates why psychoanalysis varies from country to country. So, for example, the chapter on the psychology of women presents views of prominent female psychoanalysts on female sexuality but does not differentiate among them on the basis of national differences; what we get are the analysts' views as individuals. The failure to link the various psychoanalytic emphases to pre-existing national cultural and social factors is disappointing.

Kurzweil makes a convincing argument for the inherent attraction of psychoanalysis ever since its founding—that it would provide long-sought solutions to universal issues and change society for the better. This continues to be its appeal to many on the Continent, especially in Germany. She also poses some vital questions. Is psychoanalysis viable in repressive societies? Are the actual operations of the International Psychoanalytic Association compatible with democracy? Can psychoanalytic confidentiality be maintained when psychoanalysis is paid for by state insurance and the analyst must provide documentary reports? In particular, have the German analysts—part of the German "psychoboom" (p. 217)—sold out to the authorities in return for insurance fees?

This work is appreciative of psychoanalysis yet judiciously critical of its subject matter. It is a valuable compendium of psychoanalytic theory and its application to anthropology, sociology, psychosomatic medicine, education, the psychology of women, and literature. But it still leaves the reader curious to know exactly how national cultures have influenced psychoanalysis.

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DONNA HARAWAY. *Primate Visions: Gender, Race, and Nature in the World of Modern Science*. New York: Routledge. 1989. Pp. ix, 486. \$35.00.

"It was a gesture that thrilled me"—that is how Shirley Strum described her first hands-on contact

with the baboon toddler Robin (p. 149). In a world threatened by nuclear war and ecological crisis, this tender experience is stylized to become a symbol of the healing touch of a wild animal, a touch that is accepted by the white woman scientist. This scene and the double-edged situation of women as scientists in twentieth-century primatology that it reflects determine the structure of Donna Haraway's new book.

The metaphor of salvation, the imagery of human and animal touching across the species border, is based on notions of gender, race, and nature that erase history and, in particular, the complexity of the contemporary history of science. Donna Haraway aims to make this history visible by analyzing the dense, discursive web of various fields of science, popular presentations, and political interests, a web in which the narratives of timeless origin are handed down. "The organizing story axis of the duality, nature-culture, forbids an account of historical mediation" (p. 146). Primatology, a science that defines the border between humans and animals, has from its beginnings been embedded in the realities of industrial capitalism, colonialism, and patriarchy. The alienation of humans and nature intersect symbolically, scientifically, and biographically with those borders that are marked by gender and race. "Biology is a historical discourse, not the body itself" (p. 290). Insofar as primatology has developed since World War II into a multicultural field, non-Western concepts of nature and other historical and political points of reference enter the models of "natural" man and the scientific description of primate societies. But the model of "universal man," "man the hunter," remains bound to that male bias that can only conceive of cooperation as a hierarchy of male individuals who divide up female resources among themselves.

The answers of modern biology to the question of what counts as human all revolve around modes of communication, not around history. "The line between natural organisms and constructed technical systems was redrawn in a radical way, so as to produce the cyborg as the central natural-technical object of knowledge in the last half of the twentieth century. Both organism and technology were theorized and encountered in practice as communications engineering problems, where the ontological distinction between the natural and artificial lost meaning. . . . In the universe of information the antipodes of the earthly ecosystem and the extraterrestrial space meet in a shared code" (p. 140). In this situation, the given forms of self-determination and understanding become brittle, and solutions are sought. The white women and the ape become surrogates, serving to set the stage for man's return to nature, to the origins. "Women's place is in the jungle" (chap. 11), whereas the flight of the cold warriors from nature and history into space is a male enterprise.

Haraway places feminist studies in this historical

context; thus, a position of innocence, which, untouched by power, politics, and desire, replaces the concept of universal man with universal woman, is unacceptable. Not the unreflected identification with being female but the deconstruction of categories, their shift within the theoretical framework, leads S. Altmann, L. M. Fedigan, A. Zihlman, and S. B. Hrdy to the reconstruction of human beginnings. "One story is not as good as another" (p. 348); critical self-reflection on one's own historical and political position in science makes the difference.

Haraway's analysis of primate research and the discourses woven around it is an extraordinary and excellent contribution to the history of science and to feminist theory, to primatological and historical research. It was with (feminist) pleasure that I followed her on her path of boundary crossing. But I do not share her vision of endless transgressions. In following such a vision, we are in danger of losing sight of the perceptions and knowledge that emerge from confronting our own limits—those limits not directly set from the outside, from culture and science—limits that are the painful and frightening experiences of our own lack of understanding, our foreignness, and our inabilities. Such experiences are present in Western culture, in particular in the social practice of women in the family as the private place for breakdowns and the refuge of the supposedly hermetically sealed Western individual—before his cover was publicly lifted. The experience of difference precisely in the most intimate relationships, above all in the relationship between mother and child, contrasts sharply with the reduction of women, of the female sex, to a function and means of reproduction of the "same." If one takes these inconspicuous differences seriously, then the dramatic leap into a radically different future, which Haraway hints at in her final chapter, appears as questionable as the patriarchal drama of origin.

The alternative is to recollect what has always been the double-edged orientation of feminist theory. Reflecting on the property in the self and the relations of reciprocal dependence, the tension between female orgasm as an experience of uniqueness and mothering as a potentially generalized practice—this has been the field in which women's self-discovery takes place. The project of feminist studies in science and history then reads like this: to follow the various descendants of "this female striding into history with a baby sling and a digging stick and founding language in the company of other females" (p. 228) on their way, crossing boundaries into a future in which "differentness" can be lived, because one's own limitations are not feared as death is feared. And I read Haraway's book as a long and elegant leap in this direction.

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JOHN MCCORMICK. *Reclaiming Paradise: The Global Environmental Movement*. Bloomington: Indiana University Press. 1989. Pp. xv, 259. \$35.00.

Although not without flaws, this book is an important and much-needed first attempt at recording the history of global environmentalism. John McCormick is primarily interested in the flowering of the so-called New Environmentalism from 1962 (Rachel Carson's publication of *Silent Spring* in that year is taken to define an epoch) to the first Earth Day in 1970 and the subsequent emergence of "green" politics. But, in support of his thesis that environmental problems are not new or newly recognized, McCormick examines the nineteenth-century roots of the movement. He also discusses the significance for global conservation of the internationalism attendant on World War II. Then, in the 1960s, came the "revolution" in human attitudes toward nature and unprecedented political agitation and action on its behalf.

For many readers the most significant question this book addresses will be why environmentalism emerged so powerfully in the 1960s. Drawing on the work of recognized environmental historians, McCormick offers a multipart explanation centered on fear for survival in a polluted world and changing social and economic priorities. But he could have made better use of Samuel P. Hays's *Beauty, Health, and Permanence: Environmental Politics in the United States, 1955–1985* (1987). And McCormick's suggestion of "advances in scientific knowledge" as a reason for environmentalism fails to draw on the rise of the ecological perspective. In fact, "ecology" does not even receive an index entry in this book! Yet, as Donald Worster has explained, it was this discipline's assumption of the interrelatedness of a delicate natural system on which all life depends that keyed public concern over pesticides, atomic testing, and pollution in its various forms. Surely pioneer ecologist Aldo Leopold merits more than one short paragraph and that in the context of the 1930s rather than the 1960s when his *A Sand County Almanac* (1949) inspired a generation of environmentalists.

As the slighting of Leopold suggests, this book evinces no interest in environmental ethics or (one line in the introduction notwithstanding) deep ecology. Yet these ideas had roots in Norway, Canada, Australia, Great Britain, and the United States. Failing to understand the power of moral perspective, McCormick discerns no link between civil rights, social liberations, and modern environmentalism. He does not recognize that many new environmentalists see nature (or parts of it, such as endangered species) as the latest in a series of oppressed minorities deserving liberation from an exploitative establishment. There is no indication in this book that the author has ever considered the link between women's rights and the rights of nature that interests so many philosophers and environmentalists today. Granted my own *Rights of Nature: A History of Environmental Ethics*

(1989) appeared too late for McCormick to use in his book, but the raw historical material was there for examination.

McCormick takes a sweeping approach to his subject. There are some benefits. He includes most of the names and events that made environmental history. He presents useful chronologies of environmental organizations and international agreements. Two chapters depart from the survey format. They explore in detail the United Nations Conference on the Human Environment (Stockholm Conference) in 1972 and the consequent United Nations Environment Programme. Although McCormick regards the Stockholm Conference as a watershed, he is frank about its failings and realistic about the undeniable tensions between priorities among the developed and the underdeveloped.

McCormick chose to write his first book about what his dust jacket calls "the largest social movement in history." Inevitably there are shortcomings in the work, but it is a valuable guide through a complicated subject. One senses, however, that the accelerating pace of global environmentalism will necessitate a sequel in the not too distant future.

RODERICK FRAZIER NASH  
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RONALD ROGOWSKI. *Commerce and Coalitions: How Trade Affects Domestic Political Alignments*. Princeton: Princeton University Press. 1989. Pp. xvi, 208. \$24.50.

In this book a political scientist attempts to test an economic theorem with historical data. The theorem in question is the Stolper-Samuelson theorem regarding the costs and benefits of free trade and protection. (See Wolfgang Stolper and Paul A. Samuelson, "Protection and Real Wages," *Review of Economic Studies*, 9 [1941]: 58–73.) It predicts that free trade will benefit those factors of production—land, labor, and capital—that are relatively abundant and, conversely, that protection will benefit those factors that are relatively scarce. Ronald Rogowski reasons that owners of the factors will form political coalitions in order to maximize their returns and sets out to discover if, in historical fact, that was the case.

After an introductory chapter in which he explains the theorem and its implications in some detail, Rogowski launches into the historical cases, which include a wide geographical and chronological range. He first considers "the revolutionary expansion of trade" between 1840 and 1914, when technological changes, especially the introduction of railways and steamships, acted as "exogenous forces" in much the same manner as a drastic reduction of tariffs in all countries (p. 21). He next looks at the interwar period with its dramatic shrinkage of world trade and then at the renewed expansion of trade since 1948. He does not restrict his analysis to the major trading nations

but includes Latin America, Africa, Asia, and Australasia.

Chapter 5 is the most daring and in some respects the most fascinating. In it Rogowski examines the evidence, such as it is, of the effects of the expansion of trade in classical Greece, of its decline in the last centuries of the Roman empire, and of its renewed expansion in sixteenth-century Europe. In chapter 6 he considers the implications of his findings for "other theories and conjectures in the social sciences"; chapter 7 contains a succinct conclusion.

Rogowski is no crude economic determinist, and he readily concedes that other historical forces—"ancient cultural and religious loyalties . . . wars and migrations . . . such historical memories as the French Revolution and the *Kulturkampf*" (p. 20)—might on occasion overpower the purely economic variables, and in one instance he cheerfully admits that "what the theory predicts does not come to pass" (p. 49). On the whole, however, he finds the theorem a useful tool for social and political analysis, although in his conclusion he suggests that "a more useful threefold categorization of factors for future analysis may be, not that of land, labor, and capital . . . but one of skilled labor, unskilled labor, and capital; or . . . of labor, human capital, and physical capital" (p. 178).

Given the vast scope of his undertaking, it is understandable that Rogowski relies almost entirely on secondary works for his historical information, but he might have chosen some of them with greater care and used them with more discretion. He includes, for example, undergraduate textbooks, David Abraham's discredited *Collapse of the Weimar Republic* (1981), and Michael Mulhall's outdated *Dictionary of Statistics* (1899).

Specialists will undoubtedly discover factual errors in the area of their expertise. For example, to write of "capital-intensive factories . . . of England and the Netherlands" in the sixteenth century (p. 153) and of "abundant . . . capital and labor" in northwestern Europe in the same century is sheer anachronism (p. 157). Other readers will find some of Rogowski's analogies and comparisons a bit farfetched. For example, "The democratization of Athens and Corinth resembled that of nineteenth-century northwestern Europe" (p. 162). Is it true that "there are clear parallels of political and cultural sentiment between the declining Roman landed elite and the rurally based fascism of our own century" (p. 162)? Will anyone believe that "the victory of the low-tariff forces in the Reichstag elections of 1912 . . . may even have helped impel the threatened Junker elite into the saving patriotic flames of World War I" (p. 40)?

Princeton University Press is to be commended for putting the footnotes where they belong, but—here I fault the author—there are too many of them. Those with substantive content should have been incorporated in the text, whereas those that merely cite sources could have been consolidated; supernumerals in the middle of sentences are especially annoying.

The bibliography is extensive, but its division between books and all other sources cited is not helpful in locating the sources. A misprint in one panel of figure 1.2 (p. 8) makes nonsense of the diagram.

These and other minor flaws notwithstanding, this is a stimulating and thought-provoking book that should attract a wide readership in several disciplines.

RONDO CAMERON  
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J. H. GALLOWAY. *The Sugar Cane Industry: An Historical Geography from Its Origins to 1914*. (Cambridge Studies in Historical Geography, number 12.) New York: Cambridge University Press. 1989. Pp. xiii, 266. \$44.50.

Whether or not you appreciate the adage "you are what you eat" or subscribe to the slogan "beef, real food for real people," you have to acknowledge the centrality of foodstuffs in daily life. What we eat not only pertains to regimen or to matters of personal identity but reaches to the level of cultural formation. With this in mind it is hard to think of any foodstuff in contemporary society that has penetrated to the core of these issues as has sugar.

Sweetness, sought eagerly throughout history in various forms (for example, manna, honey, raisin wine, maple syrup, and so on), became plentiful only with the advent of sugar cane agriculture in the Atlantic world. It was the sugar cane industry that not only helped revolutionize dietary habits in Western Europe (as Sidney Mintz has so aptly pointed out) but also transformed tropical landscapes (especially certain Caribbean islands) into plantations and created whole societies based on African slave labor. Not many crops have had such a profound economic, demographic, and social impact on human society in such a brief span as have the "noble" varieties of *Saccharum officinarum* L.

This saga of sugar cane from its remote origins in ancient India through its transoceanic peregrinations to its almost insidious florescence in the New World is the task before J. H. Galloway. This is not an unknown tale. The classic two-volume macrohistory by Noel Deerr covered this territory, and other more recent works—for example, studies by Franklin Knight and Manuel Moreno Fraginals on Cuba and Peter Eisenberg on Pernambuco—have expanded the history of sugar agriculture and slave culture at a microlevel. But Galloway brings a mastery of the field to bear in this brisk, sure-footed trek through the primary sources and secondary literature (including a bibliography of over three hundred fifty entries) on sugar cane growing and processing.

Although Galloway opens with a glimpse of the eastern origins of sugar cane and its manufacture and closes with the return of the industry to Asia, the heart of his story details the spread of cane agriculture from the Mediterranean world to the islands on



both sides of the Atlantic as well as to mainland South America. Focusing on patterns of production, Galloway relates when the different sugar colonies came on line, how they incorporated the lessons of their forebears, and how they distinguished themselves.

The discussions of the individual sugar-producing colonies are necessarily brief. One advantage of the lean approach is that no excess of tonnage figures weighs us down, nor must we wrestle with arcane statistical measures of sugar cane production or prices. Nevertheless, business historians will be intrigued with the case Galloway builds regarding the role of innovation, both in terms of processing technology and plant breeding, in steering the course of the industry. He complements these points with a convincing discussion of the entrepreneurial ability of the planter class in using new energy sources (steam), incorporating new machinery designs, and adapting to a nonslave labor force.

Galloway's command of the literature and long-term familiarity with the locales of which he writes lend this book a convincing air of authority. In text and in graphic (for example, the nicely drawn figure of Barbados as a center of innovation in the eighteenth century), he displays his ability to encapsulate, indeed refine, the history of the sugar cane industry to allow devotee and novice alike to savor this darkly sweet colonial precursor to what in today's world is termed "agribusiness."

J. D. GOODYEAR  
Johns Hopkins University

SAMUEL L. MACEY. *The Dynamics of Progress: Time, Method, and Measure*. Athens: University of Georgia Press. 1989. Pp. xiv, 275. \$35.00.

It is not so common for professors emeriti of English to be thirty-year members of the Institute of Management Services, and this book on standardization is written by a man who on the whole has managed to keep himself from becoming standardized. In some ways this is reason for applause, although as historians we may hesitate when this independence implies neglect of the normal standards of scholarship. It does not inspire confidence that his chapter on the history of clock technology includes multiple citations to James Burke and Jacob Bronowski (both public television personalities, whose standing among historians of science and technology may be compared to that of Will and Ariel Durant among European historians) and none at all to David Landes or Otto Mayr. On the history of chronology we would like some evidence that the author has read Anthony Grafton; on seventeenth- and eighteenth-century thinking about the age of the earth, we expect a reference to Paolo Rossi, or at least to some historians of geology. Any modern discussion of the history of weights and measures ought to be informed by an awareness of Witold Kula's *Measures and Men* (1986).

No trained historian would make the mistake of taking on faith Frederick W. Taylor's own comments on the contentment of workers under scientific management, especially when there are good modern studies of its implementation using factory records and other original documents, which present quite a different story.

There are other senses, too, in which Samuel L. Macey fails to conform to the normal standards of professional history. To his credit, his book ranges freely over centuries, countries, and areas of human experience. It gives frequent vent to opinions about the (bad) effects of labor unions and the cupidity of modern business managers, who together are given responsibility for the industrial decline of North America and Britain relative to Japan. It shifts freely back and forth between historical narration and the personal experiences of the author, thereby sometimes jerking us across two, three, or fifteen centuries.

Although inattention to the best available sources is clearly a defect, the loose and rambling flow of argument does have a certain charm. I should add that, although Macey's scholarship is weak, his instincts are generally sound, and this book is by no means a collection of historical howlers. There is too little new here to excite specialists, and the book is probably too unstructured to be useful in courses, but the interested general reader, if there remain any members of this vanishing species, could still read it with enjoyment and profit.

If the book has real scholarly value, it is in the perspective it embodies rather than in its treatment of any single topic. Macey writes about rationalization and standardization, and he pursues these themes into a wide diversity of fields. He begins with clocks and the structuring of time, then moves on to calendars, schemes of measurement, number systems, the English language, mass production, and time-work studies. He concludes, fittingly, with a section on the rationalization of human beings. This could have been done more effectively if there were less free association in the flow of argument. Macey could also have brought out more clearly the bearing of all of this on social and political power. Too often, he is content to summarize a standard popular historical account of a topic, rather than constructing a new narrative to bring out what is distinctive to his approach and what holds his book together. Nevertheless, this is a provocative juxtaposition of topics, and even professional historians could profitably reflect on their mutual relationships and implications. Interpreting rationality in terms of standardization provides a highly promising way to integrate history of science with technological, economic, and especially social and political history.

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AZAR GAT. *The Origins of Military Thought: From the Enlightenment to Clausewitz*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1989. Pp. xii, 281. \$49.95.

In his preface, Azar Gat states that he intends to show that the major currents of modern military thought, here defined as the "conceptions of military theory" and not their application, "emerged out of the cultural frameworks and the historical and philosophical outlooks of the Enlightenment on the one hand and . . . the Counter Enlightenment or the German Movement on the other." Failure to recognize the culture-bound nature of military theory, not as widespread as he claims, often has "resulted in a narrow, unhistorical understanding of its development" (p. 10).

Gat therefore divides his study into two parts. The first sketches the evolution of military theory from Machiavelli through Montecuccoli to the late Enlightenment theorists in France and Germany. Stressing an essential continuity, directed toward the discovery of a "universal theory of war" and the search for absolute, even mathematical, principles, he includes both Archduke Charles and Henri de Jomini in this section. Although brief, the observations here are perceptive. The second part surveys the military-intellectual reaction against the Enlightenment in Germany, a development caused by the surprising successes of the French revolutionary armies but also driven by Romantic idealism. Finding its first true expression in the writings of Georg von Behrenhorst, this reaction culminated in the work of Carl von Clausewitz. Enlightenment military theory, of course, did not disappear completely, and Gat briefly discusses the interactions among Clausewitz, Charles, and Jomini.

Attention then moves to the thought of Clausewitz and his interpreters. Gat claims that, at least until 1827, Clausewitz actually conceived his great opus, *On War*, as a book calling for total war, which many of his critics, notably Liddell Hart, have charged (pp. 209–10). After that date, however, Clausewitz realized that near-total war on the Napoleonic pattern could not provide the basis for a universal theory of war and shifted toward his major theme, that is, the interrelationship of war and policy. This view is debatable. Already in his earlier writings—for instance, "The Germans and the French" dating to 1807 or his "Observation on the War of the Austrian Succession," composed in the early 1820s—Clausewitz was preoccupied with the problems of political ends and military means.

Even more controversial is Gat's claim that *On War* was never meant to be a purely theoretical and philosophical work. "Much has been written," he states, "to the effect that Clausewitz totally rejected prescriptive theory," but this, he contends, is, at best, only a "partial understanding." Clausewitz, he maintains, meant to be prescriptive, and for him "theory

was by no means divorced from praxis; on the contrary it had to be translated into praxis" (p. 211).

Aware, of course, that eminent scholars such as Peter Paret and Raymond Aron have rejected this view, the author charges that Paret not only failed to place Clausewitz in his "actual intellectual context" but "totally misinterprets the essence of Clausewitz's military teaching throughout his life," and he accuses Aron of showing "cultural naïveté" (pp. 169–70). Such bold statements by a young scholar require solid substantiation, but Gat, who frequently splits some very fine hairs to support his theories, does not make a convincing case. Primarily concerned with the genesis of *On War*, and neglecting much of the larger context of Clausewitz's intellectual life, he actually places himself into much the same position that he attributes, wrongly, to Paret and Aron.

This is not to say that the study lacks merit. It is a well-conceived and broadly researched tour de force that will not only stimulate debate but also serve as a useful, often original, introduction to the historical development of Western European military thought.

GUNTHER E. ROTHENBERG  
Purdue University

JAN P. NEDERVEEN PIETERSE. *Empire and Emancipation: Power and Liberation on a World Scale*. New York: Praeger. 1989. Pp. xv, 421. \$49.95.

Jan P. Nederveen Pieterse is a sociologist, a historian, a cultural anthropologist with field experience in Africa and South America, and, as this book shows, a man of unusually wide-ranging and sophisticated scholarship. His theme is the world-wide relationship between empire and emancipation, between power and liberation, analyzed as a dialectical interaction from above and below and reaching into all dimensions of human life. For this purpose he attempts "a global approach that is kaleidoscopic in space and time, and multi-dimensional in outlook" (p. xv). His theoretical framework, subject to an extensive analysis in his opening four chapters, is derived from many sources: Karl Marx, the Marxists (including V. Lenin), Immanuel Wallerstein, Gunnar Frank, fellow Dutchman W. F. Wertheim, plus innumerable sociologists, historians, and philosophers past and present; the scope of the author's knowledge is awe-inspiring.

The main part of the book is devoted to historical inquiries ranging, with many novel conclusions, over the entire course of Western imperialism. At the outset the author deplores the bias in traditional historiography, "which exaggerates Europe's uniqueness and underrates the contributions made by other cultures, thus obfuscating the overall global process of transformation" (pp. 88–89). Subsequently, he shows how imperial domination led, dialectically, to the emancipation of the dominated. He also discusses, in often-fascinating detail, the role of class

struggle in the Western world and in imperial expansion. The relationship between racism and empire is similarly treated. His chapter "Continuities of Empire" traces the Anglo-American partnership in world domination as the culmination of four hundred years of "Western" hegemony. Other chapters deal with the survival of the North American Indians and the links between the abolition of slavery in the United States, European colonialism, and eventual decolonization.

The conclusion, "Dialectics of Empire and Emancipation," shows Pieterse's mind at work. As a critical philosopher, he first analyzes the concept of dialectics, invoking Michel Foucault, Friedrich Nietzsche, and Marx. Then he sums up his findings in terms of that concept, here and there adding, perhaps somewhat confusingly, some new material. His book ends on a humble note: "It is not possible to be conclusive with regard to the relations between empire and emancipation;" his sketches merely explore "the scope of our ignorance" (p. 381). Bewildered by the profusion of historical detail and theoretical considerations, the exhausted reader may wonder what then is the benefit of this book?

This work has its good points. It attempts a global perspective with a heightened sensitivity toward the interdependence of all factors, with a range of knowledge far beyond the scholarly norm, and with becoming humility. But it lacks literary flair; it is not easy to read. At times it seems overly theoretical. And its conclusions, while stimulating, are often dubious. Pieterse says (p. 103), to give one example, that at the time of the Renaissance "the Europeans entered the Orient as barbarians," prevailing by force of arms and the knowledge acquired from the region. But, one might ask, why did the Asians and Africans fail to acquire the skills that might have prevented European domination? Why did only the Europeans learn from others? Throughout the book the author fails to examine the unique cultural resources behind Western superiority. He also overlooks the fact that, realistically observed, emancipation implied further submission to Western skills of power; decolonization deepened the hold of Western culture over the world. In addition, one finds no discussion of fascism or communism or of the exceptional case of Japan.

Who then might profit from this impressive but difficult book? The most obvious beneficiaries are the handful of globalist intellectuals speculating beyond their depths; here is a thoughtful challenge. In addition, specialists in historical subjects treated by the author might be stimulated by his perspectives and sensibilities. And historians generally, if they could but spare the time, might let themselves be inspired by Pieterse's scholarly industry and intellectual sophistication.

THEODORE H. VON LAUE  
Clark University

DAVID C. GORDON. *Images of the West: Third World Perspectives*. Savage, Md.: Rowman and Littlefield. 1989. Pp. xviii, 214.

It is difficult to know what sort of audience David C. Gordon's book is intended to reach. It is too much a bibliographical essay—and one that assumes familiarity with the historical contexts of the authors and ideas that Gordon discusses—to have much appeal to a popular or even an undergraduate audience. But, at the same time, most of the ground that he covers will be quite familiar to area specialists, and his treatment of individual thinkers and a wide variety of African and Asian responses to European global hegemony is too abbreviated to add much in the way of new insights or approaches. The only "images" of the West that Gordon deals with in any depth are drawn from Islamic societies in the Middle East and North Africa. Chinese, Japanese, and non-Islamic African and Indian responses are mentioned in places but only in passing or as a prelude to the section that forms the core of the book: an extended typology of Muslim reactions to European imperialist domination of their societies and the continuing economic and cultural hegemony of the West in the postcolonial era.

Unfortunately, after expending considerable energy and space setting up the typology of Muslim reactions, Gordon all but abandons it. Rather than employ his typology to give coherence to the survey of mainly Muslim responses that follows, he meanders through a loosely connected discussion of what appears to be a random sample of critical themes and issues relating to societies belonging to a rather ill-defined entity that he calls the Third World (the penultimate chapter, for example, includes Japan and Israel in a discussion of "patterns of self-identification"). The topics covered range from dependency theory and the brain drain from Africa and Asia into the industrialized world to the status of women, again mainly in Islamic societies, and the anguish and anger of intellectuals caught between the "East" and the "West." Gordon includes some telling anecdotes and a wide range of opinions voiced by African and Asian writers and political leaders. But the limited space that he has to devote to topics as vast and complex as overpopulation and economic underdevelopment means that he can do little more than summarize some of the more prominent positions of "Third World" thinkers on these issues and perhaps mention the arguments of one or two authors who dispute their views.

Aside from the rather neglected typology of Islamic responses, no overarching argument or set of themes informs Gordon's choice of authors and topics that take up the bulk of the book. There is no sense of chronology and little attempt to create a context for the many authors he introduces. Contemporary writers are jumbled together with nineteenth-century thinkers; politicians and poets are introduced

and then quickly shunted off the stage. Even writers who receive several pages of commentary are handled indecisively and acontextually. In places, for example, Gordon appears wary of some of the more sweeping strictures against Western scholarship on the Islamic world associated with Edward Said and writers who share his anti-"Orientalist" stance. But Gordon merely summarizes the now-familiar critiques of Said and his allies, at times setting them against the retorts of their (presumably Orientalist) opponents. The very real problems that Said and others have raised for all who are engaged in cross-cultural research or even for those who wish to engage in meaningful intercultural dialogue are alluded to but never really confronted. Here, as throughout Gordon's survey of "non-Western" images of the West, capsule and very partial summaries are substituted for serious, in-depth analysis of the genesis and content of African and Asian reactions to Western imperialist dominance and the meaning of those responses in the postcolonial world.

MICHAEL ADAS  
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PIERRE PLUCHON. *Toussaint Louverture: Un Révolutionnaire noir d'ancien régime*. Paris: Fayard. 1989. Pp. 654. 150 fr.

From the Frenchman Louis du Hoca to the Martiniquen Aimé Césaire, propagandists have veiled Toussaint Louverture in a fog of unreality to promote their special interests for over a century and a half. Any serious biographer must first lift the mist of myth from the man. Pierre Pluchon does this without destroying Toussaint's historical importance.

Pluchon argues that the Black Liberator was a revolutionary of the *ancien régime*. This is not a contradiction of terms. Toussaint successfully replaced white with black masters to maintain the colonial order of Santo Domingo. Perhaps one should not view his action as strange because the colony provided him with his only working model of society. Democracy and popular government were but vague terms in Toussaint's world. Both were dead in France by late 1799.

Pluchon addresses several essential questions about Toussaint's record. Why Toussaint defected from the Spanish to the French in 1794 is the first of these. The author demonstrates that the Black Liberator was driven to his decision by more than the National Convention's abolition of slavery. More compelling was his deteriorating position with the Spanish. His immediate superiors, Georges Biassou and Jean François, claimed that the aspiring black leader challenged the interests of Charles IV of Spain by planning to liberate slaves, massacre whites, and control the Spanish military commandant of Santo Domingo (Don Joachim de Cabrera). Soon Toussaint expected

the Spanish to arrest him. Even the French general Etienne Laveaux expressed reluctance to accept this hard-to-control and complex rebel from the Spanish side.

Was Toussaint a great reconstructionist? This is the second essential question about the Black Liberator. Most historians accept that Toussaint was, but not Pluchon. He maintains that Toussaint's policy of inviting the return of former masters to plantations failed. Toussaint's own plantation was a model of prosperity, but most of the French habitations either remained in ruins or suffered labor shortages. Moreover, plantation profits generally benefited black leaders and not their followers. Scholars might find this interpretation more convincing if Pluchon had used the numerous U.S. accounts of Toussaint's state. Many Yankee ship captains were impressed by Toussaint's efforts at reconstruction and expressed their views in open letters to North American newspapers.

A third important question about Toussaint is his reason for surrendering to the French in 1802. Pluchon suggests that Toussaint may have been physically exhausted, even though he knew that he could defeat Charles Leclerc's army. Toussaint also ungraciously blamed the violence of the revolution on Jean Jacques Dessalines and Henri Christophe. No historian, including Pluchon, has solved the riddle of the Black Liberator's surrender. Could Toussaint have been avoiding unconditional surrender later and the certain enslavement of his people? But even this theory is shaky because, as Pluchon points out, Toussaint had agents in France who kept him informed of Napoleon's intentions toward him. Did they tell Toussaint about Napoleon's plans to restore slavery in Santo Domingo before his capitulation to Leclerc? If so, this would cloud the reasons for Toussaint's surrender even more.

Pluchon's excellent use of French sources does not extend to those in English. Moreover, he ignored much of the Haitian scholarship on the subject of Toussaint and the Haiti revolution. Thus, Pluchon might have strengthened his interesting and sometimes surprising biography of the Black Liberator by making use of works such as those by Thomas Madiou and H. Pauléus Sannon.

THOMAS O. OTT  
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GREGORY CLAEYS. *Thomas Paine: Social and Political Thought*. Boston: Unwin Hyman. 1989. Pp. xiv, 257. Cloth \$49.95, paper \$24.95.

Before his hasty departure for Paris, Thomas Paine spent several months living in a pub in a village outside London, working on part 1 of *The Rights of Man*. Last fall a local group hailed Paine as "the unsung hero of Islington" and proposed not a melody but a stately obelisk to commemorate his stay. Gregory Claeys also claims that Paine has been insuf-

ficiently appreciated and offers in tribute a fine and thorough study of Paine's political legacy. As Claeys well demonstrates, Paine's profound commitment to natural rights for all men (his radicalism stopped at the gender line) was the pivot on which his many opinions turned. Hereditary monarchy became for Paine ipso facto a denial of rights and evidence of men's debasement; the British constitution compromised the true principle of representation on which political justice rested.

By disavowing the settlement of 1688, Paine forced other English reformers to consider the grounds on which they stood. In his unabashed enthusiasm for popular political participation and his unmediated rejection of the capacity of England's leaders for self-reform, he also provoked very powerful instincts for survival in the political establishment. Claeys recapitulates in vivid detail the brutal harassment of Painites, explicating as well how constitutional loyalists answered Paine's radical formulae for reform. An astounding publishing success, *The Rights of Man* became a rights issue in itself; owning it was a crime!

Paine's goals, Claeys says, "were emancipatory as well as informative" (p. 104). The vehicle for those goals was his rhetoric. He was so powerful a word-smith that even his opponents quoted him. Paine compressed his social rage into simple, declaratory sentences that liberated the demand for equality in his readers and stripped the veil of reverence from the "crowned ruffians" and men of "no-ability" who denied it.

In analyzing the intellectual responses of those who had to find new footing in the revolutionary terrain dug up by America and France, Claeys shows the sustained vitality of the natural rights tradition, despite the competitive social theories offered by Edmund Burke, David Hume, and Jeremy Bentham. Yet the French revolution debate, he argues, did lead to a reshaping of Whig discourse along both prescriptive and utilitarian lines. Natural rights survived as the basis of working-class radicalism, only to be supplanted in the middle of the next century when industrialization undermined confidence in the efficacy of Paine's tax and welfare proposals.

Less concerned with Paine's American career than with the development of his mature political thought and its impact on English politics, Claeys nonetheless pauses long enough to criticize Paine's recent biographers, Eric Foner and A. O. Aldridge, for not sufficiently appreciating Paine's concern for virtue (p. 48). He fails to mount a serious argument here, and indeed his exploration of Paine's commercial republicanism tells against this correction. Taking Paine at his word about not reading others, Claeys neglects some important roots to Paine's thought. His ideas about the capacity of trade to promote peace and justice, for instance, were put into circulation in the late seventeenth century by Roger Coke and John Houghton, which is not to deny the originality of

Paine's reworking of these themes for the Age of Reason.

Although Claeys pays serious attention to Paine's critics, his study is unabashedly celebratory as his stirring peroration confirms. Yet it is fair to say that by taking Paine's natural rights seriously he has revealed their enormous appeal to all men and women struggling for that esteem that only a recognition of their moral worth can secure.

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JOHN J. JOHNSON. *A Hemisphere Apart: The Foundations of United States Policy toward Latin America*. (The Johns Hopkins Symposia in Comparative History.) Baltimore: Johns Hopkins University Press. 1990. Pp. x, 271. \$32.50.

The 1820s marked a decade of disillusionment—or some may contend realization—in U.S.–Latin American relations. In 1815 the fervor of Yankee support for the revolutionary movements of Simón Bolívar and José Francisco de San Martín ran high. By 1829, when Andrew Jackson entered the White House, an isolationist chasm had appeared that distanced the two continents for the remainder of the century. John J. Johnson ably reexamines the causes for that gap within the context of relations among the United States, Britain, and Latin America.

The author poses the critical question of why the pro-insurgent Yankees became so quickly indifferent to their southern neighbors. His answer revolves around six factors: monarchism versus republicanism, Catholicism versus Protestantism, internal problems within the new nations, attitudes toward the racial amalgam in Latin America, U.S. technological development and westward movement, and U.S.–British relations in respect to Latin America. The author suggests that, although each of these elements affected U.S. policy, the British chiefly determined how the United States dealt with Latin America.

The book views the period from 1815 to 1830 from a Yankee perspective—and always with a side-glance across the Pond. Johnson contends that the United States and Britain were not, as is often assumed, economic rivals. The Yankees, with little capital and few merchant houses, hardly contested British manufactures and contented themselves generally with the sale of agricultural products. In fact, the two powers agreed on most key issues, including racial and religious attitudes, trade policies, and containment of the French. Both nations realized that confrontation might damage their interests while cooperation would facilitate their respective goals—a positive economic climate for each and national security for the United States.

Only two issues emerged as potential Anglo-American trouble spots. A possible British, or even Latin American, takeover of Cuba excited intense security



and trade interests in Washington. In turn, a scheme for a hemispheric league of nations under U.S. auspices raised British hackles. Such an organization might have reduced the new republics to Yankee appendages, an unacceptable proposition in London. The idea quickly evaporated, however, largely because of Latin American suspicions of Washington's ambitions.

Johnson intends to complement, not replace, the more traditional works of J. Fred Rippy and A. P. Whitaker. His revisionist theses of a dual policy of Yankee self-interest or self-absorption and general Anglo-American cooperation are persuasive, if not altogether convincing. Certainly, Johnson is correct that U.S. policy makers distanced themselves politically from their neighbors (except for Mexico and Cuba) and relegated the Latin American states to secondary economic and diplomatic status. The study admirably blends classic sources within a framework of the social and economic climate on both sides of the Caribbean.

There is some repetitiveness of themes, however, and Johnson occasionally overstates his case: "Thus, by 1829 all thought of a flourishing trade with the new polities had vanished" (p. 106). New treaties in the 1830s with Mexico, Chile, Venezuela, and Peru-Bolivia suggest that not all policy makers had abandoned hope. Minor criticisms aside, this is a provocative work that should be read by diplomatic historians who seek a challenging perspective on this formative period in U.S.-Latin American relations.

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JOSEPH S. TULCHIN. *Argentina and the United States: A Conflicted Relationship*. (Twayne's International History Series, number 5.) Boston: Twayne. 1990. Pp. xvii, 193. Cloth \$24.95, paper \$9.95.

Relations between Argentina and the United States have never been smooth or easy. The historical roots of this "conflicted relationship," as this fine study by Joseph S. Tulchin shows, run deep. Although Argentina was Latin America's wealthiest and most dynamic nation before World War II, it never had the military and industrial power to challenge successfully the leading role played by the United States in the hemisphere. This was a reality that the Argentines, who also aspired to hemispheric leadership, consistently refused to accept. It has been this refusal, and the basic asymmetry in power, that has underlain the troubled relations between the two nations for most of the nineteenth and twentieth centuries.

The first half of this book examines the formation of the Argentine-U.S. relationship from the original controversy over the Malvinas/Falkland islands in the early nineteenth century to the difficult period of Argentine neutrality during much of World War II. The second half focuses on the postwar period, with

extensive consideration of U.S.-Argentine relations during the first presidencies of Juan D. Perón (1946–55), the impact of the cold war, the struggle over human rights in the 1970s, the disastrous war with Great Britain in the early 1980s, and Argentina's attempts to "reinsert" itself into world affairs in recent years after being something of an "international pariah" for a decade. Except for momentary periods of crisis, a dominant feature of the U.S.-Argentine relationship has been the fact that the United States has been much more important to Argentina than vice versa, a point of constant irritation to the Argentines.

The strengths of this book are many. The research is extensive and solid, enlivened by personal interviews with key players in recent events. Tulchin places his story securely within the context of domestic developments in both nations, which, given the tumultuous nature of contemporary Argentine political history, is no easy task. Showing a close familiarity with the literature on U.S. diplomatic history, he also pays proper attention to Argentina's place within the larger framework of U.S. policy toward Latin America. Argentina's relationships with its South American neighbors and the consequences of those for its dealings with the U.S. are also discussed. Tulchin writes with grace, style, wit, and power, avoiding the mind-numbing detail that often burdens diplomatic history. Although the history of U.S.-Argentine relations through the Perón period has been covered in several English-language studies, Tulchin's work casts these old developments in a new light and provides a much-needed updating of events since the 1950s.

As with any study of this scope, some questions arise. Tulchin asserts that "the conduct of relations with Argentina during the war [World War II] literally tore the U.S. government apart" (p. 63). Although Argentine policy did bedevil the State Department and led to dismissals and resignations, to say that it "tore the government apart" is to exaggerate the importance of the issue. Some consideration, too, might have been given to the possible role of the United States in the various military coups in Argentina in the 1960s and 1970s. Was the Argentine case in this regard substantially different from Brazil (1964) and Chile (1973)?

These questions aside, Tulchin has provided a serious, insightful, and lively book on the history of U.S.-Argentine relations. It is one that every scholar of the subject should consult and one that students and other interested readers can enjoy and learn from as well.

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KENNETH R. STEVENS. *Border Diplomacy: The Caroline and McLeod Affairs in Anglo-American-Canadian Rela-*



tions, 1837–1842. Tuscaloosa: University of Alabama Press. 1989. Pp. xii, 225. \$36.95.

This is the first full-scale study linking the *Caroline* and McLeod affairs and evaluating their influence on Anglo-American-Canadian relations in the late 1830s and early 1840s. Previous scholars, most notably Kenneth Bourne, Wilbur Devereux Jones, and Howard Jones, have investigated the significance of these incidents in the framework of Anglo-American relations, but no one has so thoroughly explored them in the context of international law, the conflicting motives of patriotism and political opportunism among all participants, and, more narrowly, the U.S.'s attempt to assert definitively federal authority in the realm of foreign relations. In addition, Kenneth R. Stevens provides an incisive analysis of the *Caroline* affair's role in the history of international law.

Although Americans today assume that the Canadian-American border has been ever peaceful, such was not the case a century and a half ago, when violent clashes along the Niagara frontier threatened war between Great Britain and the United States. The most provocative of these border skirmishes occurred in late December 1837, when a band of Britons and Canadians, angered at U.S. support for and participation in a festering Canadian rebellion, attacked the steamboat *Caroline* in American waters, killing at least one American and destroying the vessel. The attack dangerously escalated tensions along the Canadian-U.S. border.

The *Caroline* raid did not lead immediately, however, to a full-blown Anglo-American diplomatic crisis. British foreign secretary Lord Palmerston disdainfully ignored American protests. It was more than two years before an official British acknowledgment of the affair was received in Washington, by which time the dispute had been further complicated by the arrest in New York in November 1840 of a Canadian, Alexander McLeod, on the charge that he participated in the *Caroline* raid and murdered an American. The arrest aroused Palmerston to action. His government defended the raid as an act of state, and hence participants were immune from prosecution on criminal charges. War or peace hung in the balance on the basis of a state court's verdict in a murder trial, dramatically illustrating the precarious nature of the federal government's control of the nation's foreign policy in the early republic.

Stevens deftly examines this unresolved constitutional issue concerning ultimate authority over foreign relations in a federal system. The dispute pitted President John Tyler, erstwhile champion of states' rights who favored national primacy in this instance, against Governor William H. Seward, a nationalist who argued for the primacy of state authority in settling the issue. In the end McLeod was acquitted, thus averting the threat of an Anglo-American war. According to Stevens, however, Seward intended all along to pardon McLeod if the jury found him guilty.

Inexplicably, the bitter controversy arose because Seward's intentions were never clearly conveyed to the administration in Washington.

The easing of tensions produced by the McLeod acquittal paved the way for the Webster-Ashburton Treaty of 1842, which resolved a number of other Anglo-American disagreements. To avoid future federal-state disputes that impinged on the nation's foreign policy, Congress in 1842 passed the Remedial Justice Act, which asserted federal authority in such cases. Arguably of comparable historical significance for the development of international law was Daniel Webster's articulation during the dispute of the "Caroline Doctrine." Webster asserted that, in order for a nation to justify a preemptive attack, it had to prove a "necessity of self-defense, instant, overwhelming, leaving no choice of means and no moment of deliberation." In a fascinating survey, Stevens traces the way in which, in the twentieth century, a number of nations have employed Webster's doctrine of self-defense as justification for their actions.

This well-researched and well-written study is a valuable contribution essential to those who seek a clearer understanding of the impact of the *Caroline* and McLeod affairs on Anglo-American-Canadian relations and of the significance of these events to the determination of federal authority over American foreign relations.

EDWARD P. CRAPOL

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MAUREEN E. MONTGOMERY. *"Gilded Prostitution": Status, Money, and Transatlantic Marriages, 1870–1914*. New York: Routledge. 1989. Pp. ix, 342. \$37.50.

Between the end of the Civil War and the onset of World War I, increasing numbers of wealthy American women married titled European men. Perhaps no contemporary has probed the psychological motives underpinning these unions or the cultural conflicts they embodied as deftly as Henry James. Maureen E. Montgomery's goal in this book is to explore the historical context that supported the creation of Isabel Archer, the young heroine in *The Portrait of a Lady* (1881), whose New World innocence was ultimately transmuted by her extended exposure to Old World experience. But Montgomery notes that the Isabel Archer type was nowhere as popular an image in England as that of the status-hungry heiress trading dollars for a title and thereby undermining aristocratic values.

American commentators, after a brief flurry of post-Civil War pride, were also critical of these alliances, linking the sale of native daughters to the flow of capital out of the country.

The overriding theme in this book consists of the ways in which the tensions between lineage and wealth were played out not only between England and the United States but also within both cultures.

The concept of status anxiety is central to the entire analysis. Noting that the arrival of American brides coincided with the opening up of the British upper classes, Montgomery argues that the British landed elite projected their deepest anxieties about the stability of their class onto these often beautiful, richly attired, and highly visible foreign women when the real threat to their status emanated instead from new, nonlanded British wealth.

The scope of this work, which juxtaposes critiques of Anglo-American marriages against quantitative data about the marriages themselves, is ambitious. Not only does Montgomery endeavor to track shifting attitudes toward wealth and lineage in both England and the United States, but she also relies on both cultural and quantitative analysis. Although the design is admirable for its comparative perspective and its methodological complexity, the results are disappointing. After delineating the stereotype of the glittering American heiress marrying into British high society, Montgomery denounces it as a cliché. Yet it is the cliché that frames her questions about the social reality. Thus, Montgomery expends far too much energy on the question of whether or not British peers married Americans for their money, although she acknowledges that the data will not yield a definitive answer. After examining the financial status of the sixty British peers and the forty younger sons of peers who married American women between 1870 and 1914 and after comparing them with two control groups, she can only speculate that marriage for purely financial gain was rare.

Insisting that both the heiresses and their spouses were unfairly maligned, Montgomery tends to view the wives as Isabel Archer types who, within the constraints of class and gender, aimed to be more than merely ornamental. Admitting that their marriages reflected the need of a new, moneyed elite to assert its claims to social status, she nonetheless underscores the avenues to political influence provided by transatlantic alliances. If their careers as British political hostesses were invariably dependent on their husbands, marriage into the top echelons of British society, she avers, provided American women of the leisured class with a far broader scope of action than would have been available to them at home.

Many readers will not be convinced. Still, flaws notwithstanding, Montgomery's treatment of the social shifts that facilitated Anglo-American marital alliances should be of interest to both women's historians and social historians.

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STEPHEN R. ROCK. *Why Peace Breaks Out: Great Power Rapprochement in Historical Perspective*. Chapel Hill: University of North Carolina Press. 1989. Pp. x, 220. \$29.95.

Classically trained historians will wonder at this effort to explain why Britain, France, and the United States "found" each other as allies before 1914 and agreed that the German empire was the enemy. Stephen R. Rock is a political scientist who has retraced the diplomatic record of the pre-1914 period. His purpose was to determine why some nations found reasons to bury animosities (Britain and France in 1904; Britain and the United States after the Venezuela arbitration) and others raced into conflict. His not-so-hidden agenda when this book was written was to identify factors that might alter the cold warrior confrontation into an arrangement less conducive to global annihilation.

Rock draws from well-known diplomatic memoirs and documentary collections to support his argument that peace was possible where nations had heterogeneous economic and political interests—that is, their interests complemented each other sufficiently to overcome destructive competition—and homogeneous value systems. Thus, despite a legacy of baiting, British and American interests were sufficiently diversified and complementary to encourage rapprochement when political circumstances motivated London and Washington to minimize hatreds.

One major problem with this work is that events have outpaced Rock's cautious predictions for U.S.-Soviet rapprochement. His remark "nor can one expect the Soviet Union to renounce socialism, particularly since Marxist-Leninist doctrine serves to legitimize the existing . . . regime" (p. 152) exemplifies the problem of prediction. As of this writing, the Soviets seem committed to supporting the United Nations embargo against Iraq, restructuring their domestic institutions, and dissolving the Warsaw Pact. Who knows what will exist when this review is published? If Rock's examination of the conditions for rapprochement seem outdated for the U.S.-Soviet situation, what point is there in rehearsing the "origins of World War I"—again?

Although Rock offers no surprises to the historian, the importance of this book is in the question that he poses. Instead of assuming that a series of crises produced the inevitable belligerent outcome, Rock asks readers to consider the options and environments that shaped peace and diminished hostilities. But, because he has undertaken a new approach, it would have been far more persuasive if he had treated the forces producing foreign policy less as unbending abstractions and more as the conscious interests of particular groupings. Along with most scholars of diplomatic history, Rock assumes that the so-called national interest is, in fact, truly national. He can only do this by ignoring the disparate social groupings within the states under consideration and assuming that the vision of the foreign office reflects the desires of entire populations.

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WAYNE S. COLE. *Norway and the United States, 1905–1955: Two Democracies in Peace and War*. Ames: Iowa State University Press. 1989. Pp. xii, 221.

Were books given a Parental Guidance Suggested rating for a lack of violence, hostility, or even tension, this study would surely qualify. This might discourage a reader not excited by two democracies being civilized toward each other, yet on closer reading, only the Norwegians are without sin.

In a slender volume about a slender subject, readers discover incidentals, such as the lack of a sewer system in the Norwegian capital, so that American diplomats lacked water closets until 1911. In World War I, the British, for fear that gaps in the blockade would allow food to slip through to the Germans, put severe limitations on Norwegian imports. In Wayne S. Cole's neat phrase, Norwegians "realistically faced the fact that as a weak state contending with a major world power, Norway had no practical alternative but to acquiesce" (p. 30). The United States on entering the war toughened the trade restrictions, until Norwegians had "the practical alternative" of starving or accepting Allied control of their trade.

In the interwar period, anger was quietly expressed by Norwegians who wished to immigrate to America. One commented on "the hollowness of American morals," for example, how immigrants were neatly fleeced of their money. Impressions got worse with the depression and the American concern with an immigrant's "likelihood of becoming a Public Charge." It became much easier for a German Jew to be permitted into America than a Norwegian. American consular officials were not merely discourteous but brutally uncivil (p. 60). On a legislative level occurred the bitter dispute about the whale oil tariff, insisted on by a Congress pressured by peanut and cottonseed oil interests.

In World War II, German occupation brought more pressures from friendly America, to the extent of halting any relief clothing in 1940 for fear that it might benefit the Germans. Lack of consideration occurred also in the niggardly response to the Norwegians, who appealed for American ships to replace some of the 44 percent of Norwegian shipping lost by March 1943. The famous Allied effort to halt heavy water production, which Germany might have used for the production of an atomic bomb, resulted in Allied bombers missing the plant but killing twenty-two Norwegians living nearby. One wonders whether any compensation was ever paid, or was America as niggardly toward Norwegians as toward the imprisoned Japanese-Americans? When the Germans left and the Russians remained so near, relations with America become dully smooth again, as the once-occupied Norwegians were willing to modify their traditional unmilitary style of life and join the North Atlantic Treaty Organization.

The author has clearly mastered the subject and the big world's picture, which is consistently inserted

to put the lesser bilateral issues into perspective. A major lesson is that the American democracy, to protect its own interests, whether in peace or war, can be quite uncivil. Having even a democratic "Big Brother" has its problems. With the Norwegians always accommodating, one might conclude that smaller democracies behave with more civility than do large ones. Power still corrupts. Democratic power corrupts democratically.

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ALICE WEXLER. *Emma Goldman in Exile: From the Russian Revolution to the Spanish Civil War*. Boston: Beacon. 1989. Pp. xv, 301. \$24.95.

"This book is about the last twenty years of the American anarchist Emma Goldman, who was deported from the United States to Russia at the height of the Red Scare of 1919" (p. 1). This is Alice Wexler's second book of a two-volume biography. It ably completes a project begun with *Emma Goldman in America* (1984) and compares favorably with Richard Drinnon's earlier biography, *Rebel in Paradise* (1961).

Wexler's project is to make sense of this extraordinary woman's life. Most of our attention has remained focused on Goldman's years before 1920 when, Wexler argues, "the most dramatic years of her life were yet to come" (p. 2). Wexler comments that one of the ironies of Goldman's life is the thinness of her record in America and the rich documentation of her life in exile—"mountains of letters" (p. 4). Wexler has mined those letters in a variety of archives to offer us new sources and Goldman's own words and voice.

The book covers Goldman's life from 1919 to 1940 and is organized in three parts. "Ruptures" traces Goldman onto the SS *Buford*, the Soviet Ark, and into Bolshevik Russia. "Recoveries" considers Emma's well-traveled exile in Berlin, Paris, St. Tropez, London, Toronto, Chicago, and Nice. "Beginnings and Endings" covers the death of Goldman's longtime comrade, Alexander Berkman, her contact with the Spanish revolution, and her death in May 1940.

Goldman initially welcomed the opportunity to visit Russia to witness the revolutionary spirit of the people undampened by either starvation or cold. She lived for a time in Petrograd, visited Moscow and Archangel, and toured the Ukraine. She spoke with high-level Bolsheviks, anarchists, and Ukrainian Jewish intelligentsia. Goldman was disturbed by what she saw; writing from an anarchist position but for "the liberal intelligentsia" in America, Goldman described her reactions. By seizing power in October, the Bolsheviks had halted the revolution and were in the process of creating a bureaucratic, authoritarian state and returning the country to capitalism. In an article in the *New York World*, later reprinted with an editorial change in the title as *My Disillusionment with*

Russia, she criticized the Bolsheviks for "creating a Leviathan far more stultifying and dangerous than any government before them" (p. 65). "The inequality, repression, misery, and especially the terror were largely internally and politically caused, an inevitable outcome of Bolshevik ideology" (p. 73). Wexler concludes that Goldman had "judged Russia through a narrow lens, offering a highly selective portrait," and that she "omitted any real economic analysis" (p. 75).

Goldman's second confrontation with revolution and Bolshevism came in Spain a decade and a half later. She spent three months there during her first visit in 1936 and was "a valuable witness, since so few of those writing about the war had any comprehension of the anarchists whatsoever" (p. 206). She returned to London, volunteered to organize an information bureau, and offered her assistance as a fund raiser and propagandist. She returned to Spain a year later for a two-month stay as the anarchist position deteriorated under Francisco Franco's final offensive. Then in her sixties, Goldman sided with the anarcho-syndicalists, against the Spanish Communists. Her position was, as always, principled but unconsciously haunted by what she had seen in the Soviet Union. Wexler is careful in identifying and evaluating Goldman's changing positions on revolution, violence, and war.

Exile for Goldman had created some opportunities and destroyed many others. This book is much more than a political exegesis; a vibrant, passionate anarchist shines in these pages. Following the works of Richard Drinnon and especially Paul Avrich, the heroic biography has become the quintessential genre for anarchist historiography; Wexler's study of Goldman's life in exile is a careful, concise, and well-written addition to that genre.

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THEODORE S. HAMEROW. *From the Finland Station: The Graying of Revolution in the Twentieth Century*. New York: Basic Books. 1990. Pp. xvi, 386. \$24.95.

Readers who judge Crane Brinton's *Anatomy of Revolution* (1938) a perceptive interpretation of the dynamics of modern revolutions will find Theodore S. Hamerow's inquiry into twentieth-century revolutions similarly rewarding. The author acknowledges Brinton's work to be his inspiration in writing this comparative history of four major radical revolutions of this century. The qualifier "radical" is key to his undertaking, because he excludes national or fundamentalist (Muslim) movements. He chooses to study exclusively the new regimes in Russia, China, Vietnam, and Cuba, labeled in most works "communist." The term "communism" is, however, completely absent from his book (it does not even appear in the index), for his approach is structural and institutional, not ideological or cultural. Leaders and activ-

ists may have thought that they could control their revolutionary undertakings, but in the author's view they were tragically mistaken.

In Hamerow's comparative perspective, the revolutionary process is more like a giant wheel than a linear movement. His revolutionary stages (each a chapter) begin with the "decline of established authority" and the "emergence of a revolutionary movement." After a passing inquiry into "public opinion" (but not into social classes), the reader moves through the "overthrow of the old order" via a "united front" to "radical dictatorship" and the "heroic decade of the new order." The last stage, which is a sort of return to origins, consists of "dictatorial power" and "social elitism." This "graying of the revolution" offers no redeeming virtues or promising new paths. Ideals vanish as careerism infects the political oligarchy and disillusionment spreads among the people. Begun in protest against the old regime, the new state drifts in an apparently inevitable process back into a similar ossified condition. Hamerow closes by citing approvingly the dismal end to George Orwell's satirical novel *Animal Farm*.

Any evaluation of this very ambitious foray into comparative (world) history should ask two key questions. Does it offer an interpretation sufficiently broad to encompass the four revolutions? Is it a comprehensive account of the revolutionary process itself? I have to answer both questions in the negative. The resemblance between Hamerow's schematic revolutionary scenario and the Russian experience is obvious. China's evolution from empire to people's republic does not fit well the proposed prerevolutionary stages, however, and Cuba's peculiar path to "socialism in one island" might as easily be explained by Fidel Castro's reaction to the international context of the 1960s as by some inherent dynamics of revolutionary regimes. This history is disappointing also in failing to take account of the importance to radical revolutions of class conflict, a subject of much thoughtful work by Russian historians, and it omits an analysis of the role of state power, an issue carefully explored in Theda Skocpol's comparative study of French, Russian, and Chinese revolutions (*States and Social Revolution* [1979]).

The dramatic new ending given these past years to the revolutionary scenario of Soviet Russia (and Eastern Europe), unforeseen and unpredictable in the conceptual approach offered here, suggests flaws in the methodology itself. Hamerow's study is in a way a product of that time when resigned pessimism characterized most Western studies of communist regimes in their period of "stagnation." A depersonalized, deterministic history such as that presented here attributes, in my opinion, far too small a role to agency in the political and social dynamics of the revolutionary experience. Hamerow's description of stages in the evolution of these regimes provides a suggestive model by which to trace the particular history of individual revolutionary states. It does not, however,



offer meaningful new perspectives on the tormented history of twentieth-century communism.

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ANTHONY SHORT. *The Origins of the Vietnam War*. (Origins of Modern Wars.) New York: Longman. 1989. Pp. xvi, 347.

As Marc Bloch might have done, my first reaction to the title of this book was to ask what Anthony Short means by "origins"? Does he want to indicate the beginnings? Or would he rather talk about the causes? Whatever the intention, this book rightly places the origins of the Vietnam War in the initial colonization of Indochina by France in the nineteenth century, an event that the author studies in a short prologue. His main account, however, starts in the first chapter, with French colonial policy over Vietnam from the 1920s to the end of 1946 when the Franco-Vietnamese war officially began. The origin of that war obviously lodged in the French obstinacy to reconquer their colony at any cost. From chapter 2 until the end of the book (chapter 7), the focus shifts to the role played by the United States and to the origins of that role. Short argues that a source of the American-Vietnamese war was the refusal of the United States to guarantee the terms of the Geneva agreements. Indeed, right after the signing of the accords, the United States did whatever it could to evict the French from South Vietnam. American officers started to train the South Vietnamese army. American arms and ammunition as well as heavy military equipment were introduced into South Vietnam in flagrant violation of the accords. The regime of Ngo Dinh Diem was created and propped up and South Vietnam turned into a separate and sovereign state. Moreover, the South Vietnamese government was goaded into refusing to hold general elections to reunify the country, because, in the opinion of Secretary of State John Foster Dulles, South Vietnam would have lost them. As records show, the U.S. objective was "to avoid having such elections" (p. 192). Subsequently, U.S. intervention extended further and further into Vietnamese affairs, as it had to, to help South Vietnam fight against the insurgency of the National Liberation Front. Ultimately, the United States reached the point of unilaterally making vital decisions, including the ones to dump the Diem family, to bomb the North, to introduce ground troops. The last decision made in 1965 started the second Vietnam war.

Short certainly does an excellent job of following events as they unfolded on their way to their inescapable resolution. The French would not have accepted anything less than the reestablishment of colonial rule in Indochina after World War II, and the Vietnamese, overexcited by the success of their revolution,

could not have settled for anything short of independence and unity. Similarly, the United States—after 1954, after Geneva and the "loss" of China—could not, in the pursuit of its so-called security, tolerate a communist regime in South Vietnam. Later on, after South Vietnam's inability to save itself from defeat became plain, the United States decided to go to war against North Vietnam "for its own purposes," that is, to avoid a humiliating defeat (p. 316). As for the Vietnamese, after 1956, "North Vietnam and the National Liberation Front were going to war, as Vietnamese, to unite Vietnam under the leadership of the communist party. The United States were going to war, as Americans, to prevent that happening" (p. 330).

I would like to bring up here two minor points. First, I am surprised not to see either in the author's bibliography or in the footnotes any direct mention of the microfilms that have made available so many declassified documents of the National Security Council, the State Department, and the Central Intelligence Agency. Second, given Short's expertise in the affairs of the Malay world, I had hoped to read enlightening comparisons and contrasts between Vietnam and Indonesia and Malaysia. As it is, readers are left hungry by the few and superficial allusions to these two other countries.

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PAUL FUSSELL. *Wartime: Understanding and Behavior in the Second World War*. New York: Oxford University Press. 1989. Pp. x, 330. \$19.95.

Being shot at and hit, and shooting to kill and hitting, are extreme human experiences, in ordinary times known only by criminals and police, in extraordinary times, such as 1861–65 and 1914–18, known by a significant percentage of an entire generation of young men. Paul Fussell has had both experiences. A distinguished literary scholar, Fussell is winner of the National Book Award for his widely read and highly praised *The Great War and Modern Memory* (1975). He holds the Donald T. Regan Chair of English Literature at the University of Pennsylvania. As a twenty-year-old lieutenant in World War II, he led a rifle platoon in the 103rd Infantry Division in Europe and was severely wounded in the spring of 1945.

Although Fussell never uses the personal pronoun, parts of this volume are autobiographical, at least in the sense that only a veteran could have written them. Fussell's descriptions of combat, fear, and human degradation in war rank with the best produced in this century. "Combat is torture," he writes, "and it will reduce you, sooner or later, to a quivering wreck" (p. 281). He traces the "dawning and dreadful realization" that comes to all combat infantry: first, the soldier rationalizes that "it *can't* happen to me. I am



too clever/agile/well-trained/good-looking." Second, another rationalization, "It *can* happen to me, and I'd better be more careful, . . . take cover/dig in/keep extra alert at all times, etc." Third comes the realization that "it *is going to* happen to me, and only my not being there is going to prevent it" (p. 282).

Fussell's overpowering descriptions of the horrors of combat make one understand why generals prefer to attack with green troops rather than with veterans: a veteran infantryman is a terrified infantryman. Odds are high that the veteran will have experienced the humiliation of knowing a fear so sharp and overwhelming as to cause him to lose control of the sphincter muscle or bladder and foul his pants. The only thing that keeps him going is another possible humiliation, "fear of shame" (p. 4), that is, of looking the coward in front of his buddies or letting his buddies down.

There is much more to this book than a lot of bang-bang, but Fussell's insistence on forcing readers to feel some sense of the reality of combat is its great contribution. It is a part of his overall theme (here he quotes Walt Whitman after the Civil War); "The real war will never get in the books," not even Fussell's own book. Insofar as World War II was the greatest single event in world history, it could hardly be otherwise; the totality of that conflict can never be captured in a book or in ten thousand books.

Fussell's aim is to provide some description of "the psychological and emotional culture of Americans and Britons" in the war. He says his book is "about the rationalizations and euphemisms people needed to deal with an unacceptable actuality." He feels this is necessary because "the Allied war has been sanitized and romanticized almost beyond recognition" (p. ix). He wants us to know the truth.

But of course he can give us only part of the truth, colored by his own emotional reactions to what he experienced. He is bitter, over many things. He was sent into combat with weapons that were inferior to those of the enemy. American and British propaganda was so full of lies and hypocrisy as to leave him sick to his stomach. Much of the book is about the literature of the war; he gives the impression that he regards the Great War as a good war because it produced a great literature, whereas he looks on World War II as a bad war because it produced an inferior literature.

There are many gems. The chapter "Rumors of War" deals with the role of rumor in war, the needs rumors fill, the harm that they do. Two other chapters, "Chickenshit, An Anatomy" and "Drinking Far Too Much, Copulating Too Little," are as accurate and insightful on the experience of being a soldier as anything ever written in English and wonderfully funny as a bonus. Anyone who has ever been in the service knows the term, but no one has ever defined chickenshit as well as Fussell. Chickenshit is "behavior that makes military life worse than it need be: petty harassment of the weak by the strong; open scrim-

mage for power and authority and prestige; sadism thinly disguised as necessary discipline; a constant 'paying off of old scores'; and insistence on the letter rather than the spirit of ordinances. Chickenshit is . . . small-minded and ignoble and takes the trivial seriously. Chickenshit can be recognized instantly because it never has anything to do with winning the war" (p. 80). The army, Fussell complains, "brings out the worst in everybody" (p. 82).

Fussell certainly sees it that way, but by no means does he speak for all sixteen million American servicemen. For many, even including some combat infantry, the army brought out the best in them and in their buddies. Fussell will have none of that, and his refusal to count their reactions as part of the psychological and emotional culture of the war is a major weakness in this book. Having described the terrified, the embittered, the angry, the vicious, as well as the poets and the novelists, better than anyone before him ever has, he simply ignores the millions for whom the war was the greatest and most significant event of their lives. Their view is as authentic as his; they, too, deserve to be described, analyzed, understood.

If Fussell had undertaken to write about their war as well as his, he would have avoided the intense controversy and criticism that this book has brought forth. Most reviewers have been as furious with Fussell as he is with the war. "What are we fighting for?" Fussell asks in his chapter "The Ideological Vacuum" (p. 134). His answer is that the war was "devoid of ideological content" (p. 136).

This is preposterous. Citing examples of Allied wrongdoing does not destroy any Allied claim to a moral superiority over the Nazis and Japanese. Fussell ignores completely such matters as who started the war, who invaded whom, who wanted to conquer and control and destroy, and who wanted to defend and resist and save.

A small point makes the big one. Fussell has a section describing the distribution of paperback books to American and British soldiers. There were hundreds of titles, from the Greeks through eighteenth-century British authors to Hemingway and T. S. Eliot. By contrast, German troops got copies of *Mein Kampf* and Goebbels's speeches. No Schiller, no Goethe, no Remarque. It is amazing that a professor of literature does not see, right there, the difference between the two sides.

Fussell argues that, because the war was empty of meaning, there was no emotion, no exhilaration, anywhere when it finally came to an end. It was just over, thank God. That is simply not true. Winston Churchill got it right when he remarked that the Nazi surrender brought forth "the greatest outburst of joy in the history of mankind."

But, although this is a flawed book, it is an important and exciting book. If the parts on the literature of the war are too narrow and too reminiscent of freshman English courses, if the sections on the

meaning of the war are perversely wrong-headed, the descriptions and analysis of what it was like to be in the army in World War II, and in combat, can only be described with a superlative—they are the best the war has produced.

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ALLAN BÉRUBÉ. *Coming Out under Fire: The History of Gay Men and Women in World War Two*. New York: Free Press. 1990. Pp. xiii, 377. \$22.95.

The virtues of Allan Bérubé's fine book may not be immediately apparent, given its familiar interpretation, methodology, and politics. As John D'Emilio pointed out in *Sexual Politics, Sexual Communities* (1983), World War II forced an awareness of emerging gay and lesbian subcultures on the part of their members, observers, and antagonists and enhanced the conditions making such subcultures possible. In terms of methodology, Bérubé works techniques common to gay and lesbian history. In his political and interpretive sympathies—toward gays and lesbians themselves, of course, and toward homosexuality as a "social construction"—Bérubé also resembles most scholars in the field. Such similarities are hardly surprising in a small field, initiated amid widespread hostilities, whose practitioners share much.

But Bérubé powerfully demonstrates, especially with his impressive use of military and medical records, what others have only suggested. His sensitive treatment of the psychiatric profession, for example, shows the ambiguities of substituting medical for moral and religious judgments of homosexuality. Reforming psychiatrists helped lead the military from a focus on the criminal act of sodomy to an emphasis on homosexual personality types and from imprisonment to discharge of gays and lesbians. But such changes, incompletely effected anyway, only sometimes minimized severe treatment, while more systematically stigmatizing gays and lesbians as a deviant class. A minority of liberal psychiatrists, convinced that gays performed as well as other soldiers (as later studies affirmed), did not prevail.

Bérubé's title aptly suggests both choice and coercion among gays and lesbians during the war. Wary of binary oppositions ("closeted" or "out," gay or straight), Bérubé unfolds complex spectrums of compromise and courage, sexual attitudes and practice, official policy and combat realities. Sprinkled throughout are telling details of how wartime circumstances changed social space: with female entertainers barred from combat zones, gay male soldiers in full drag ("Georgia from New Georgia," as one was called) took their place, with official sanction.

The book has limitations, some deriving from the limits of source materials. Comparative analysis involving other countries would have been helpful. Attention to social divisions, particularly of class, is

sometimes weak: the experiences of officers and enlisted personnel are largely conflated, with homosexuality among the higher ranks almost unmentioned. Bérubé says little about wartime transformations in civilian work and social space. Despite his broad subtitle, his focus is on military life and institutions and, hence, because far more men than women donned uniforms, on men more than on women. And Bérubé, sensitive to his story's complexities, does not succinctly summarize the broad changes underway.

His book makes possible such a summary, however. The military's overwhelming need for personnel encouraged it both to formalize its antihomosexual policies and, sometimes, to overlook, tolerate, even sanction the presence of gays and lesbians. The sum of wartime changes within and beyond the armed forces was neither liberation nor repression but sharp shifts in policy, language, and social space that accentuated both impulses, placed each on a collision course with the other, and led "to a redefinition of homosexuality as a political issue" (p. 253). In turn, politicization set the stage for both postwar antigay witch hunts and fledgling gay activism, pioneered by veterans indignant at how their government treated them. No longer could the issue remain confined to the realms of private anguish or elite control. As Bérubé knows, politicization owed much to broader historical forces as well, but he powerfully argues the transforming dynamics of World War II. And as with other recent work, the tone and language of his book—confident, undefensive, precise—indicate the maturation of the field and its pertinence and accessibility to nonspecialists.

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RONALD L. FILIPPELLI. *American Labor and Postwar Italy, 1943–1953: A Study of Cold War Politics*. Stanford: Stanford University Press. 1989. Pp. xvi, 288. \$32.50.

Ronald L. Filippelli's book joins a growing number of studies that extend the field of foreign relations beyond a focus on government-to-government diplomacy. Filippelli skillfully blends a survey of the international politics of the emerging cold war with an intricate presentation of politics within the labor movement, factional maneuvering within the Italian-American community, and the complicated postwar Italian political scene that was torn by differing labor ideologies. American postwar policy toward Italy emerged, in part, out of a transnational web of labor and ethnic connections that predated the war.

Filippelli shows that leaders of the Italian-American labor movement realized well before the State Department that the working class would be critical to Italy's postwar political complexion. Labor foreign policy, by affecting the political orientation of states with strong communist parties (France as well as

Italy), could substantially shape the international political arena. With the defeat of fascism, prominent labor leaders, especially those in the Italian-dominated International Ladies Garment Workers Union (ILGWU), feared that communists would triumph not only on the shop floor in Italy but also in the antifascist coalition that stood to inherit political power and in the international labor organization, the World Federation of Trade Unions. Trying to limit communist influence in Italy seemed a logical part of a broader campaign against radicalism in unions at home. During and immediately after the war, then, leaders in the Italian-American labor movement, operating mainly within the American Federation of Labor (AFL), strengthened their ties to the right wing of the Italian Socialist party. They also worked successfully to increase support for Italian anticommunist Christian groups and moderate socialists among members of the U.S. Office of Strategic Services and the State Department, even though government policy ostensibly supported the communist-dominated Italian General Confederation of Labor (CGIL), the united front of the antifascist trade unions.

In 1947 and 1948 the State Department, especially as it hammered out the Marshall Plan and forged other links in its anticommunist strategy, fused its own policies to those of the Italian-American labor leaders and also to the most organized anticommunist institution in Italy, the Catholic church. American government and labor leaders (both AFL and Congress of Industrial Organizations [CIO]) supported Catholic unions that broke away from the CGIL and joined a new United States-sponsored international labor organization, the International Confederation of Free Trade Unions (ICFTU). The triumph of business unionism and the purge of leftist elements at home fit nicely with the design of the Marshall Plan and what Charles Maier has called the "politics of productivity" abroad.

Filippelli notes that the close identification between U.S. policy and the American labor movement overseas ran counter to the American-sponsored supposition that "free" labor movements should have no connection to government. Just as the U.S. government used certain Italian Americans and unions as instruments of cold war policy, so, too, did certain powerful Italian and labor leaders use the government's cold war concerns to advance their own particular anticommunist agendas. The author also suggests that the union-government anticommunist alliance, which began so clearly in postwar Italy, has continued to be "one of the least known and most important facets of American policy" (p. xiii) throughout the world.

Although Filippelli labels the American policy of working with labor conservatives in Italy a "success" in terms of State Department goals, he emphasizes that the Italian labor movement became one of the weakest in Europe and, despite the promise of productivity, made few gains during the late 1940s and

early 1950s. American policies helped discipline Italian labor but did not exert comparable pressure on Italian business elites to pass on the fruits of the increased productivity resulting from labor restructuring. Filippelli's thesis seems consistent with, but does not overtly address, Michael Hogan's "corporative" interpretation of the Marshall Plan. Unfortunately, the focus of this book allows little space for attention to the transnational interplay of business elites and leaves one eager to know more of the business context that would match this labor history.

This study is a welcome contribution to the relatively neglected topic of international labor politics. Researched in governmental, union, and personal papers in both the United States and Italy, it adds detail and further complexity, without challenging essential interpretive outlines, to previous studies of postwar Italian-American relations by John L. Harper, James Miller, and Federico Romero.

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JOHN W. YOUNG. *France, the Cold War, and the Western Alliance, 1944–49: French Foreign Policy and Post-War Europe*. New York: St. Martin's. 1990. Pp. 309. \$45.00.

This first detailed and comprehensive history of French postwar foreign policy uses recently opened archives in the Quai d'Orsay and the Public Records Office (but not, unfortunately, the U.S. National Archives). It is a most welcome and densely written addition to an increasing volume of literature on the origins of the cold war that stresses the importance of the Europeans as historical actors independent of the Americans and Soviets. Young sets out to explain how France, which entered into an alliance with the USSR in December 1944 to the great discomfort of its Anglo-Saxon allies, came to join the North Atlantic Pact, which was directed against Moscow, a little over four years later. To some extent this question is a nonstarter; as the author explains, France was a liberal democracy, geographically and culturally in the West, capitalist, and a colonial power. In that sense there was no real "choice" for France between East and West. Nevertheless, the French made a pretense of acting as a bridge between the Anglo-Saxons and the Soviets while doggedly pursuing their own aims in occupied Germany until 1947 when the emerging cold war forced them to choose sides. Young is particularly effective in explaining the nature of France's recognition of the need to rely on Washington: its political weakness in the face of internal Gaullist and Communist threats, economic dependence on the United States for raw materials and the dollars to finance the Monnet Plan for economic modernization, and need to deal with the British and Americans to get a German settlement even remotely responsive to French security needs.

Unfortunately, these first two reasons precluded satisfaction of French needs in the third: the price of Marshall Plan assistance was French acquiescence in British and American plans to reconstruct Germany.

Young raises a more interesting question in the conclusion: why was France unable to pursue a policy that was at once more self-assertive and responsive to internal considerations of national pride? Somehow de Gaulle managed to project France more on the world stage from 1944 to 1946 than his successors, who had much more of a recovered nation to work with. Before turning to external constraints on French policy to answer this question, Young would have done better to examine more some of his own assertions. First, he exaggerates the extent to which Paris favored a more pacific approach to Moscow than that of the British or the Americans; if the French saw no wisdom in standing up to the Soviets over Berlin, it was because they feared defeat. They were prime movers in the establishment of the North Atlantic Pact and were much more fearful than Washington ever was of a Soviet sweep through Germany to the Pyrenees. French diplomats warned the Americans on several occasions that as bad as the Nazi occupation had been, a Soviet occupation of France was bound to be worse, because it included in its plans the extermination of the entire national elite. Second, Young minimizes French importance to American aims in the postwar era; Paris became the linchpin of American policy, the centerpiece of American military planning, and France the country on which Washington relied in its hopes to secure European integration. To be sure, this became clearer after Young's period; the Schuman Plan closes his narrative but opened an era of American reliance on France that only came to a close with the failure of the European Defense Community in 1954. Finally, in what may be the chief weakness of the book, Young neglects French colonial policy. It was the squandering of Marshall Plan aid on costly colonial wars that prevented France from achieving more in terms of economic growth and consequently national self-assertiveness. France chose dependence on Washington at the same moment it opted to attempt the colonial conquest of Indochina. The coincidence was not accidental.

These caveats aside, readers will find much that is new and of interest in Young's account. French postwar policy in Germany has never been so carefully and fully explained, and the existence of plans in the Quai d'Orsay for the integration of Germany with a federal Europe, expressed as early as working papers of 1944 and pursued avidly in 1947 and 1948 by Georges Bidault, the predecessor of the better-known Robert Schuman at the foreign ministry, will appear surprising. Particularly welcome is Young's demonstration that the removal of the Communists from the French government in May 1947 was the consequence of internal developments, not American pressure. But, by 1948 and 1949, American relations

with France took place on at least three levels: diplomatic, political (involving meddling in internal French political squabbles), and economic. By dealing almost exclusively with the diplomatic level, Young falls short of presenting French foreign policy in all of its complexity.

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WOLFRAM F. HANRIEDER. *Germany, America, Europe: Forty Years of German Foreign Policy*. New Haven: Yale University Press. 1989. Pp. xviii, 509. \$29.95.

The events in Eastern Europe in 1989–90 have brought us to a watershed in the historiography of postwar Europe. With historians and political scientists as unaware as most political leaders of the impending collapse of Communist hegemony in Eastern Europe, almost all scholarly treatments of the period 1945–89 were based on the assumption that the power relationships of the cold war, or the balance of terror between the blocs, were relatively stable and could be taken as the given context for any analysis. Now, however, we are forced to ask not only whether such massive analyses as Wolfram F. Hanrieder's comprehensive survey of the first four decades of West German foreign policy should have at least glimpsed the coming changes but, even more important, whether their failure to do so has invalidated their very treatment of the period before 1989, which might have been accepted had the confrontation between the blocs continued unchanged.

Hanrieder's failure to foresee events that occurred in the year of his book's publication has certainly led him to some incorrect conclusions. For example, he says that Chancellor Konrad Adenauer's assumption "that with the passage of time the balance of power between the two Cold War blocs would shift in favor of the West . . . which would induce the Soviet Union to settle the German question on terms acceptable to the West" was "false" (p. 8). In 1990, Adenauer was on the way to being justified. Perhaps more important, the author declares that "Europeans are aware that the sources of the Cold War are deeply embedded in contending political, geopolitical, and ideological systems, and that the division of Europe can be overcome only gradually, if at all" (p. 138). It was, of course, the sudden freedom of East Europeans to reject a system that the majority had only tolerated because of coercion that led to the rejection of the Communists, not the least in East Germany. Here is the danger of speaking of systems rather than people.

As historians, however, we must not blame others for failing to predict the future but ask whether they have skillfully interpreted the past. One can hardly criticize the author for his admitted pessimism. Composing ninety-three pages on the military strategy of the blocs and their painful progress toward avoiding



Mutual Assured Destruction would be sufficient to make any observer what Hanrieder calls "the chronicler of past and current strife and herald of future discord" (p. xviii). But, at times, the pessimism may have affected his views. He sees Chancellor Willy Brandt engage in his policy of Ostpolitik out of "resignation" (p. 20), accepting the present and merely looking for possibilities of change in an unpredictable future. Here, however, we reach the crux of the interpretation of Hanrieder's rich and intriguing book, in which the finest writing occurs when he treats these two contrasting chancellors. Was Adenauer correct in his assumptions and Brandt a defeatist too soon? Or had the policy of confrontation exhausted its benefits by 1969, making it necessary to gain the small foothold within the Eastern bloc through which internal dissension might be fomented, if only by greater personal contacts? The debate is only beginning, however, and Hanrieder provides considerable help in placing such questions within the broader context of West Germany's international relations, above all with the United States.

The opening section on security is long, full, technical, and overly skewed toward a consideration of U.S. diplomatic and military strategy. The limits of German independence of policy or even of participation in alliance policy making are shown to be growing constantly, from the days of dependence under the High Commission in 1949–55 to the effective intervention in short-range and intermediate-range missile disputes in the 1980s. The second section, which is perhaps the most interesting, deals with the evolution of West German policy toward East Germany and the broader question of German unification. Between Adenauer's successful Westpolitik in the 1950s and Brandt's seemingly successful Ostpolitik in the early 1970s, Hanrieder sees the 1960s as troubled, a period of diplomatic isolation on the German question and of corrosive conflicts over economic policy. The third section on political economy argues that, largely as an aftermath of the occupation, Germans expressed their political intentions in the language of economics. Their wealth, for example, became their diplomatic weapon in dealings with the East European countries in the 1970s, and it gave them the power to push European integration to embrace new monetary agreements. The final section is a clear, chronological account of the internal political changes that influenced West German foreign policy, most notably the shift of the Social Democrats in the 1960s to an acceptance of the Christian Democratic policy toward West European unification and of the Christian Democrats in the 1970s and 1980s toward Brandt's Ostpolitik. *Treatment of the late 1980s, however, is very thin.* The negotiation of the Single European Act and preparation for the unified market in 1992 in the European Community is passed over quickly. Even more regrettable, Soviet president Mikhail Gorbachev is mentioned briefly only three times.

There is no bibliography, but the end notes are a dream: 117 pages, or almost a quarter of the book. As an overview of the vast literature on West German foreign policy, they are invaluable.

This book, as the author states, is not "a diplomatic history of the Federal Republic or even a history of its foreign policy" (p. ix), which is a pity, because Hanrieder is one of the principal experts on West German foreign relations. For those who are already acquainted with the main features of the Federal Republic's foreign policy, Hanrieder provides an enormously learned synthesis, filled with suggestive insights into the relationship between events, into the vagaries and the occasional brilliance of political leadership, and into the complex interplay of politics, economics, and the preparation or avoidance of war.

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GORDON H. CHANG. *Friends and Enemies: The United States, China, and the Soviet Union, 1948–1972*. (Modern America.) Stanford: Stanford University Press. 1990. Pp. ix, 383. \$25.00.

A book that conforms with one's own interpretation of events as well as one's preferences in the writing of history is easy to like and easy to review. Gordon H. Chang's work is such a book. It is a study of elites, of officials or consultants who either made or recommended policy toward China and the Soviet Union in the period from 1948 to 1972—individuals who left an extensive documentary record of their ideas.

The central thesis of the work is that American policy makers did not function as simple-minded anticommunist ideologues, motivated by belief in a monolithic Sino-Soviet bloc; instead, they acted on the assumption that differences would emerge between the two communist nations that could be exploited to America's advantage. Debate in the Truman administration focused on which stance to take: whether to attempt to split the Chinese Communists from the Soviets by wooing the Chinese—in effect following the British example—or to adopt a tough, uncompromising approach toward the People's Republic of China (PRC). A leading proponent of the former approach was Chester Bowles; a leading proponent of the latter was Charles W. Yost, a foreign service officer who played a part in defining Truman's China policy. Yost's idea, followed in large measure by Dwight D. Eisenhower and John Foster Dulles, was that forcing the Chinese into dependence on the Soviet Union was the surest way of driving a wedge between the two. For all of their rhetoric to the contrary, Dulles and Eisenhower did not endorse the overthrow of the PRC (although they came close to attacking China with nuclear weapons during the Taiwan Strait crisis) but sought, rather deftly, to split



the Soviets and Chinese through initiatives to the Soviets.

President John F. Kennedy extended the Eisenhower initiative to the point of seeking to isolate the PRC through détente with the Soviets. Indeed, Kennedy, increasingly aware of the Soviets' disenchantment with their ally, apparently went so far as to propose a joint Soviet-American attack on the developing Chinese nuclear capability—a suggestion that Khrushchev rejected, surely to the great regret of his successors less than a decade later.

In Chang's opinion, improved Soviet-American relations played a part in radicalizing China's internal politics in the mid-1960s; militant revolutionism was Mao Zedong's way of distancing China from the Soviet Union. American involvement in Vietnam became, in turn, the Kennedy-Johnson reaction to China's militancy. Richard Nixon, ironically yet perspicaciously, moved away from alliance with the Soviet Union against China toward dialogue and improved relations with the Chinese as a way of gaining leverage with the Soviets.

Chang explicates this story convincingly, with verve and with intimate knowledge of the sources, which are, for the most part, American (the private papers of policy makers and U.S. government documents). His work seems flawed in only two areas: in his conclusion that the United States might have developed a working relationship with the PRC if it had not, early on, forced the Chinese into a choice between the United States and the Soviet Union; and, second, in his brief foray into race as an explanation of American policy. On the first of these points, a student of this triangular relationship must surely conclude that, American mistakes aside, the Soviet Union was resolutely unwilling in the early days of the Sino-Soviet alliance to allow the Chinese much latitude for friendship with the United States. On the second, he fails to demonstrate to what extent, if at all, American racial prejudice may have informed American action. He suggests, in the same sentence (p. 208), that both racial prejudice and support for Chiang Kai-shek predicated U.S. hostility to the Chinese Communist party. Race did not prevent attachment to Chiang, nor did it prevent improved relations with Japan, given the interest in containing the Soviet Union. A final point: while American officials engaged in racial stereotyping to demonize the Chinese enemy, the Chinese engaged in class stereotyping to demonize the American enemy.

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MARK A. RYAN. *Chinese Attitudes toward Nuclear Weapons: China and the United States during the Korean War*. Armonk, N.Y.: East Gate. 1989. Pp. xii, 327.

Soon after taking power in 1949, the leadership of the Chinese Communist party (CCP), faced with U.S.

nuclear threats in the Korean War, quickly developed a relatively sophisticated understanding of the destructive capacity and military utility of nuclear weapons, as well as the psychological and political dimensions of their use. Even more impressively, the CCP pursued a domestic policy toward the weapons threat that allayed civilian and military fears, while promoting a view internationally that may have helped deter the United States from turning to the nuclear option.

Mark A. Ryan reaches these conclusions based on his close study of U.S. documentary evidence and Chinese materials published primarily in the late 1940s and early 1950s, including previously neglected articles in the Chinese mass media, civil defense and cadre manuals, and specialized journals. The views expressed in these sources and Chinese actions, he argues, show that the CCP leadership not only took nuclear weapons seriously but also developed an attitude of "integrated realism" about them in general and about Washington's nuclear capabilities in particular (p. 180). Ryan disputes earlier accounts that see the Chinese as naive and ignorant, if not reckless, about atomic warfare until the mid-1950s.

Ryan is perhaps most impressed with Chinese nuclear doctrine and philosophy. According to him, the CCP leadership, even before seizing power, appreciated earlier than most some of the far-reaching implications of the new military technology. The Chinese even developed an understanding of the "deterrent value of Soviet nuclear weapons" "more fully and publicly than the Soviets themselves" (p. 76). And the CCP leaders were more sensitive to the ethical issues of nuclear war than U.S. leaders, who possessed "large blindspots" (p. 185).

Especially interesting in this study is the information about early Chinese civil defense thinking and measures and the possible influence that strategic planning had on the Chinese urban landscape. The Chinese closely studied Western scientific studies, military manuals, and other public material on nuclear technology and warfare.

Ryan also contributes to longstanding controversies: he finds no evidence to sustain Chinese charges that the United States used chemical weapons in Korea, but he does not entirely rule out the possibility that limited U.S. biological warfare was conducted. (Some Western specialists have linked Chinese accusations about chemical and biological warfare to propaganda efforts to inhibit U.S. use of nuclear weapons.) As to Eisenhower's claim that his nuclear threat helped end the war, Ryan concludes that the brandishing of nuclear weapons did not appreciably affect Chinese decision making.

Ryan's capable book is unfortunately marred by overly long quotations and endnotes, which make for tedious reading at times. He also touches but lightly on China's own crucial decision to develop nuclear weapons. He did not have access to Chinese archival material or oral interviews, nor did he draw exten-

sively from the recently published retrospective Chinese material on the Chinese nuclear program. Nevertheless, until Chinese archives become accessible and we can learn what Mao Zedong and his comrades were thinking about nuclear issues, works such as Ryan's contribute significantly to our understanding of the early strategic planning of what is today the world's third largest nuclear power.

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W. WARREN WAGAR. *A Short History of the Future*. Afterword by IMMANUEL WALLERSTEIN. Chicago: University of Chicago Press. 1989. Pp. xiv, 323. \$24.95.

Here is what lies ahead for us, down to Earth Festival Day, 2200, when this transcript of Peter Jensen's holofilm "A Short History of the Future" appeared.

The severe world-wide depression that began in 1995 brought such universal public turmoil that the two superpowers, the United States and the USSR, were led to test each other's intentions for further empire and came within an ace of unleashing nuclear war. Chastened by the near crisis, the two superpowers then entered into a pact, which permitted at once dramatic arms limitations and mutually acceptable extensions of their respective empires ("zones of legitimate special interest" [Wagar, *Short History of the Future*, p. 39]). The principal weakness in the new arrangement proved to be overlapping responsibilities for order in the Middle East, where Israel was assigned to the care of the United States and the rest to the USSR and its surrogate Syria. The proximate cause of the global nuclear war that broke out in 2044 (World War III) was thus, predictably, the miscalculation of the USSR in seeking to drag Israel into its zone and the entirely unexpected decision of the American president to order nuclear attack.

The underlying cause of the war, however, was the abrupt and devastating collapse of the capitalist system, beginning in 2032. Before its collapse, capitalism had accomplished the systematic impoverishment of the whole world outside of Europe, North America, and Japan. At the same time, the constant pace of change, which capitalism required, undermined the authority of all belief systems, all forces that traditionally restrained the satisfaction of appetites, notably, the restrained religions and the institution of marriage and family. There was the virtual dissolution not merely of religious belief but of all belief—general "credicide" (p. 11).

World War III took an estimated 5.8 billion lives. But that was not the end. Human life was painfully resumed, and, in an astonishingly short time (about twenty years), a new world order was in place—the Commonwealth, a truly global, socialist order, proclaimed in 2062, which we owe to neo-Leninists, clear-eyed about the need for force against recalcitrant superstition and tribalism. This global socialist

order—unlike the ersatz socialist politics of the twentieth century—was thoroughly scientific and almost wholly benign, permitting a wide range of liberties, extending to the right of political opposition. When the time came for it to wither away, it yielded with very little fuss to opposition elements, led by the Small party, proponents of decentralization. By the end of the twenty-second century, the human race was making all of its decisions at the neighborhood level.

W. Warren Wagar tells us that his "threefold schema . . . replicates the apocalyptic paradigm in Christian eschatology" (p. xi), but these phrases seem a grandly allusive way of saying that he has located the ultimate historical crisis in Israel. "Apocalyptic paradigms" aside, "the working hypothesis is the Marxian expectation of the future demise of world capitalism leading to proletarian socialism and finally pure communism." In short, we have not yet seen the history that Marx, Engels, and Lenin predicted and that the Communist regimes have been claiming to have been fulfilling since 1917. But we shall.

This book is one product of a newly entrenched academic field, "future research" or "future studies." (There is a six-page bibliography at the end, which includes at least six journals, thirty books of "methodology," and scores of "scenarios" of the type of the present book.) Although "having no methods peculiar to itself," future studies "claims knowledge of what is unknowable" (p. ix). Christians, who take their eschatology neat, will recognize the claim, and the method and the product, as forms of Gnosticism. Empiricists will want to know about the point of departure in contemporary (that is, real) fact and will probably follow as far as the extrapolations seem probable (whatever "probable" may mean to the particular empiricist). Such readers should note that the ink dried on the text in January 1989 and that Peter Jensen's political narrative begins with the death in bed sometime in the 1990s of Nicolae Ceaușescu (Wagar, *Short History of the Future*, p. 35), which "uncorks explosive forces" in Romanian society, resulting in a reformist regime, which is subsequently crushed by the Soviet leader of the day so that its example does not spread. From this moment, the USSR goes from strength to strength. If you can follow that far, you can follow all the way.

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#### ANCIENT

BEZALEL BAR-KOCHVA. *Judas Maccabaeus: The Jewish Struggle against the Seleucids*. New York: Cambridge University Press. 1989. Pp. xvi, 672. \$125.00.

This book, a thoroughly revised and expanded version of *The Battles of the Hasmoneans*, published in Hebrew in 1980, is a study of the military aspects of

the revolt of Judas Maccabaeus (or Judah the Maccabee) against the Seleucids (166–160 B.C.E.). The body of the book (pp. 194–402) consists of extensive discussions (introduction, translation, commentary) of the eight passages from 1 Maccabees that portray Judah's military activity. This is preceded by a series of thematic studies on Hellenistic military techniques, the Seleucid army, and the tactics and armaments of the Jewish army (pp. 3–148) and is followed by a series of appendixes and excursus on points of detail (pp. 413–572). The whole is enlivened by numerous maps and plates and is rounded out by indexes and bibliography.

Bezalel Bar-Kochva is also author of *The Seleucid Army* (1979), and his expertise in military matters is apparent. (Israel is perhaps the only country in which virtually all male academics have served in the army and most have experienced combat.) Bar-Kochva carefully assesses the topographical and tactical aspects of each battle and supplements the meager information of 1 Maccabees with the data provided by 2 Maccabees, Josephus, and what is generally known of warfare in the Hellenistic period. Bar-Kochva's goal is to debunk some of the mythology (some, not all—Bar-Kochva, too, as the first sentence of the prologue shows, sees Judah's struggle as "heroic") that has accumulated around the name of Judah the Maccabee, mythology that begins in 1 Maccabees itself. Bar-Kochva argues that Judah was not always outnumbered by his foes, that his troops were not always ill trained and hastily assembled irregulars, that he was not always successful in the open field, and that his greatness lay not in his military successes but in his ability to create, organize, and sustain a standing army. I am not competent to assess Bar-Kochva's military arguments (unlike Bar-Kochva, I have no military experience and am not intimately familiar with the terrain where the battles took place), but his arguments based on the literary character and theological motives of 1 Maccabees are persuasive and cogent.

In his commentary on the passages from 1 Maccabees, Bar-Kochva discusses more than just military matters. He affirms the widely held view that 1 Maccabees is historically more reliable than 2 Maccabees. He often reconstructs the lost Hebrew original of 1 Maccabees, and many of these reconstructions are clever and plausible (for a good example, see pp. 252–53). He is able to marshal both classical and rabbinical evidence to clarify various points (for example, see pp. 368–70). The book is not perfect, of course. The English is occasionally a bit stilted; an Israeli nationalist perspective, inherited from J. Efron, Bar-Kochva's teacher, is sometimes in evidence (p. 59, n. 101 and elsewhere); Bar-Kochva conceives of Jewish religious law as "the halakah" (pp. 371, 474–99), a decidedly religious conception predicated on the assumption that Jewish religious law was (and is!) a unified and universally accepted system (hence, the definite article); Bar-Kochva could have done

much more to refute, or at least discuss, the suggestions of Jonathan Goldstein in his commentary on 1 Maccabees (1976; Goldstein's commentary on 2 Maccabees appeared in 1983). But these objections do not detract from the fact that Bar-Kochva has succeeded in accomplishing what he set out to do.

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JOSIAH OBER. *Mass and Elite in Democratic Athens: Rhetoric, Ideology, and the Power of the People*. Princeton: Princeton University Press. 1989. Pp. xviii, 390. \$39.95.

For the modern as well as the ancient observer, the relative stability of the democratic government of the city-state of Athens over nearly two centuries has presented a difficult problem: under what circumstances did the people, or "masses," come to accept as legitimate the political leadership of the elite? Some scholars have appealed to the favorable conditions created by the existence of the empire in the fifth century or by the practice of slavery, others to the alleged stabilizing effects of a posited "middle" class or of a "face-to-face" society. Rejecting as inadequate all such attempts at explanation, Josiah Ober finds a key in the particular mode of communication practiced between leaders and people. The rhetoric of the popular assembly, council, and courts is especially pertinent, because in perhaps all of the extant speeches of the so-called Attic orators, an elite politician or litigant found himself confronted by a mass audience more or less representative of the citizen population as a whole. So the challenge to the elite orator was to find a means whereby he might persuade a group whose interests were often fundamentally opposed to his own. Neither Max Weber's charismatic emotionalism or Moses Finley's instrumental rationalism is held adequate to explain the orators' overall record of success. Rather, it is in ideology that Ober situates the common ground eventually occupied by elite and mass.

Detailed discussion of the oratorical texts explores the workings of Ober's hypothesis with regard, first, to class. By "class" the author means the division between the rich (the "leisure" class) and the poor (who were compelled to work for a living), which was only partly healed by various forms of redistribution—a "dissonance" remained between the prevailing egalitarian political ideology and inequalities in property holding. That such tension never erupted into open conflict Ober attributes to successful attempts to mediate the potential polarization by symbolic means "at the level of ideology" (p. 205). In the courts, for example, wealthy private litigants might ally themselves with poor jurors by purporting to be "poor," not in an absolute sense but relatively in comparison with others richer than themselves, or by

treating the predominantly poor jurors as though they were in fact well off. Because poor jurors were willing to "suspend disbelief," bonds of mutual interest were created, leading to what Ober calls "ideological consensus." Alternatively, wealthy politicians might exploit the positive aspects of wealth, demanding the jurors' sympathy in gratitude for their public benefactions. The tactic proved successful when the speaker convincingly maintained that his wealth had been used (and so used in a spirit of generosity) to the benefit of the city-state as a whole. Similarly, where not class but status was at issue, and despite the general distrust of elite behavior, aristocratic attributes proved acceptable in political leaders on the condition that they, like the property of the wealthy, were first made subject to the people's ideological hegemony.

Jettisoning the traditional approach to this subject in terms of constitutions and laws, Ober has opened up new and potentially productive areas of investigation. That, in doing so, he has himself already advanced our understanding so far puts all students of the classical world in his debt.

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VIVIENNE GRAY. *The Character of Xenophon's Hellenica*. Baltimore: Johns Hopkins University Press. 1989. Pp. x, 219. \$32.50.

Vivienne Gray's study of the *Hellenica*, a history of the Greek states from 411 to 362 B.C., is a worthy companion to several thought-provoking books on the works of Xenophon that have appeared in recent years, notably William E. Higgins's *Xenophon the Athenian* (1977) and James Tatum's *Xenophon's Imperial Fiction* (1989). Eschewing the well-worn question of when the *Hellenica* was composed and in what stages, if any, she promises a close reading of the text in order to establish its literary character and historiographic purposes. She lays her cards on the table in the very first pages: "I believe that the value of written history is the discovery of the mentality of the writer and the society that produced the writer" (p. viii).

Following a preface and introduction, the three main sections of the book correspond to three narrative modes frequently employed by Xenophon: conversationalized narrative (a mixture of reported informal speech and authorial narrative), formal speeches, and straight narrative. Within each section, introductory and concluding chapters frame detailed analyses of specific episodes. The epilogue pulls together the main threads of the study.

Gray cannot detect a single theme or story line that holds together the *Hellenica*, but she does point to certain recurrent concerns of the historian. Xenophon emphasized displays of moral qualities and achievements such as leadership, loyalty, and friend-

ship. He was far less interested in mechanically chronicling the military and political events of the era than in bearing witness to the punishment of Sparta for impiety in breaking an oath to allow other Greek states their freedom, and in documenting the moral regeneration of the Athenians who learned the virtues of gratitude and forgiveness from the adverse experience of losing the Peloponnesian War. This moral focus accorded with the common view of the ancients who regarded Xenophon primarily as a philosopher.

Despite the fact that the *Hellenica* begins where the text of Thucydides abruptly breaks off, Gray presents a forceful case for the predominant influence of Herodotus rather than Thucydides on Xenophon's historiography. This can be seen in the structure of the work, which interweaves natural blocks of material; in its style, which employs language and sentiments appropriate to a given occasion and actor; and in the use of speeches, which primarily serve to characterize the protagonists. It is a far cry from the rigid seasonal chronology, uniform language, and analytical function of speeches in the pages of Thucydides.

Gray finds that characteristic elements of structure and style and a consistent set of concerns operate throughout the work and takes this as a strong argument for the unity of the *Hellenica*. She also maintains that recurrent patterns found in conversations, speeches, and narratives raise serious doubts about the authenticity of this material. The hand of Xenophon is present everywhere, and it is virtually impossible to separate out "true facts" from the dominating patterns.

One could take issue with some of the editorial decisions, such as the very brief general index (one-and-a-quarter pages) and sporadic inclusion of the original Greek text. Such quibbles, however, do not detract from the value of this book. There are few surprises here, but if Xenophon turns out to be typical of his age, then the reader comes away with a clearer sense of what that means. The *Hellenica* is shown to be a more interesting book than is usually thought, and Xenophon is seen to have exercised a greater degree of control over his material than he is usually given credit for. Many of Gray's contentions do not submit of proof, but let us hope that she has initiated a dialogue in which others soon will take part.

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THOMAS WIEDEMANN. *Adults and Children in the Roman Empire*. New Haven: Yale University Press. 1989. Pp. xii, 221. \$27.50.

This excellent and (in the best sense) idiosyncratic book is the first full-length analysis of ancient Roman concepts of childhood and attitudes toward children.



It will not be the last, because "the family" is now squarely in the sights of Roman historians and the place of children is commanding increasing attention.

Since Philippe Ariès's *Centuries of Childhood* was published and translated from French nearly thirty years ago, there has been widespread acceptance of his argument that there was no concept of childhood as a distinct phase of life in Europe before the seventeenth century. On the basis of this argument, extreme judgments have been passed on the history of childhood. Recent scholarship has shown how misleading these views are, and Thomas Wiedemann's book contributes strongly to such scholarship. He shows the many ways in which children were distinguished from adults—in public and private life, in legislation, office holding, education, religion, and, to some extent, in the work force. Although he claims in his first chapter, "The Child in the Classical City," that the ancient evidence presents a "largely negative picture" of children, most of his book undermines this assertion (p. 42). His own engagement with the subject yields a vibrant picture of a society in which children had many roles to play.

The emphasis is on change. Wiedemann is concerned with "what makes a child a person" (p. 200), tracing criteria from the bearing of arms to office holding, inherited wealth and status, precocious educational attainments, and, finally, Christian salvation. During the second century, Roman law paid much attention to the needs and rights of children. From the end of that century, age barriers to office holding were beginning to disappear, but this was discontinuity rather than continuity. Children nine years old (and younger) as patrons of cities and magistrates were surely ciphers, representing their families' standing and ambitions, but not real, autonomous persons. And it is arguable that with infant baptism, and thus full admission of even the youngest children to the Christian community, real distinction between child and adult had gone. Wiedemann takes a positive view of this development in his final chapter, "Equal in the Sight of God."

The chronological range, from the first to sixth centuries A.D., is a strength. Some generalizations across the centuries, however, are questionable, for example, those on child beating, which use the sixth-century St. Benedict to describe "the normal practice of a Roman household" (p. 28). Recent work by Richard Saller (in Beryl Rawson, ed., *Marriage, Divorce, and Children in Ancient Rome* [1991]) argues that, in at least the first two centuries A.D., Roman fathers distinguished clearly between slaves and their own free children in administering corporal punishment.

The literary evidence is well presented in two chapters, "Biography and Panegyric" and "Pagan and Christian Letters," but the most original chapters are "Citizenship and Office Holding" and "Learning for Adult Life." The latter deals not only with formal education and job training but also with the wide

range of socializing influences and activities of a Roman child's life.

The publisher has been generous with twenty-two plates that considerably enhance the text.

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## MEDIEVAL

RICHARD KIECKHEFER. *Magic in the Middle Ages*. (Cambridge Medieval Textbooks.) New York: Cambridge University Press. 1990. Pp. x, 219. Cloth \$39.50, paper \$9.95.

Because of magic's inevitable involvement with both Christianity and science, it has been of perennial interest to scholars and students of the Middle Ages. Within a compass of two hundred pages, Richard Kieckhefer has presented a thorough summary of medieval magic in its many connections and manifestations. He describes the medieval inheritance from classical, Norse, and Irish paganism and from the Islamic world.

Within the late medieval period, Kieckhefer rightly distinguishes the common tradition of magic from its intellectual counterpart. An intellectual tradition was "found among both clergy and laity, among both nobles and commoners, among both men and women" (p. 17). Because magic was so ubiquitous, Kieckhefer denies the existence of an identifiable class of magicians. Despite the great range and multiplicity of practitioners, however, the forms and content of magic in the common tradition were strikingly similar. Here we find the use of charms, incantations, amulets, talismans ("talismans, unlike amulets, have written words or at least letters inscribed on them" [p. 77]), exorcisms, and sorcery.

By contrast, intellectual magic came to the late Middle Ages in the great wave of translations from Arabic to Latin during the twelfth and thirteenth centuries. The major beneficiaries and practitioners of intellectual magic were university-educated students and scholars. Although many aspects of the common tradition could be found in intellectual magic, astrology and alchemy formed its core, which is why Kieckhefer describes the practice and principles of these two basic disciplines (pp. 120–39). Intellectual magic was usually intended not for popular dissemination but for the use of initiates. Intellectual magicians, especially alchemists, feared that popularization of their subjects would not only diminish their aura of mystery but also reveal their secrets to irresponsible practitioners who would bring disgrace on true adepts.

Another crucial dichotomy that courses through this volume, and which forms the core of medieval magic, is the distinction between natural and demonic magic. Indeed, taken together, these two alternatives define the very concept of magic in the Middle Ages.



Excluded from either alternative are God's actions and the normal, manifest operations of the forces of nature. But virtually all else is included. Thus, under natural magic medieval scholars included the occult forces of nature, which radiated from the celestial region and entered terrestrial bodies. These external powers enabled bodies compounded of the four elements to perform actions that were beyond the mere capabilities of their ordinary elemental constituents. Commonly cited examples of such external powers were the forces that caused a magnet to attract iron as well as certain powers of plants and gems that afforded protection against a variety of diseases.

As the term implies, demonic magic involved the ritual invocation of demons by a magician. Because demons belonged to the fallen angels who had rebelled against God, any magician engaged in such practices was deemed to have rejected the Christian religion and was often considered a heretic. The legal or moral acceptability of any act of magic was determined by a judgment of whether demons had been invoked, that is, whether the magician had practiced natural or demonic magic.

Kieckhefer has produced an admirable scholarly synthesis of virtually all aspects of magic in the Middle Ages. For the first time, scholars and students of the humanities have an eminently readable, reliable, and helpful guide into this ubiquitous, albeit irrational, activity.

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RICHARD HODGES. *The Anglo-Saxon Achievement: Archaeology and the Beginnings of English Society*. Ithaca: Cornell University Press. 1989. Pp. xi, 212. \$44.50.

Richard Hodges has produced another tour de force, aptly advertised on its dust jacket as a "provocative new interpretation of English history in the first millennium." The book presents a wealth of up-to-date, fully referenced archaeological information, integrated into a major work of economic and social history. It is well produced and informatively illustrated, and it has a good index but lacks a consolidated bibliography. The criticisms that follow should be read in the context of my general praise for Hodges's achievement.

Readers should note Hodges's subtitle, for an understanding of archaeological evidence is essential to the evaluation of his historical interpretations. He provides a succinct review of these forms of evidence (pp. 1-9). Hodges's sources of inspiration include Alan Macfarlane's *Origins of English Individualism* (pp. x-xi, 2-4), Jack Goody's work on the family and marriage, and Keith Hart's study of commoditization (pp. 196-200). Hodges is at his most readable on the early Anglo-Saxon period (chap. 2); history plays

such a small part that he assigns the *adventus saxonum* to "a prehistoric episode" (pp. 22-23). Chapter 3 considers the effect of Christianity on society, economics, and settlement patterns, and chapter 4 devotes considerable space to coinage and the emergence of *emporia*.

The influence of Carolingian Gaul (chap. 5) and the Viking threat or competition (p. 155) set the scene for the pivotal argument of the book, when "an outstanding individual (King Alfred) made history by grasping the circumstances of his choosing" (p. 149). In the highly contentious chapter 6 ("The First English Industrial Revolution"), Hodges attempts to justify his assertion that Macfarlane's view of the "contribution of the English to modern capitalism"—itself hotly disputed—"should be sought in the first, not the second, millennium" (pp. 2-3). I cannot accept Hodges's idea that the Industrial Revolution of the eighteenth century "provides an index against which to measure the history of the later ninth and tenth centuries" (p. 151). I concur (with E. L. Jones) that the Industrial Revolution was based on an unprecedented ecological windfall, involving major overseas conquests, and must therefore deny Alfred a "place in history as the architect of the first English Industrial Revolution" (p. 196).

Chapter 7 provides a convenient survey of the whole book and includes an excursus on commoditization (pp. 198-99). The concluding statements lack the bullish confidence of earlier chapters; Hodges simply stresses the continuity of English government from Ine to the later Middle Ages and the evolution of a class structure over the same period that permitted "the beginning of a proto-capitalist age" after the Black Death (p. 202).

A different, and somewhat unpleasant, Anglocentric theme runs through Hodges's book in a rather inexplicit manner. It appears first on page 3 ("I seek to investigate the makings of the English identity") and is reinforced by the title of the final chapter, "The Genesis of a Nation." Hodges endows the "English" with "identity" (pp. 44, 200), "individuality" (p. 186), "singular propensities" (p. 9), and even "Englishness" (pp. 2-3). By page 191, they have become a "race," despite a casual reference to "the British (or Anglo-Saxons as they had become)" on page 190. My Celtic hackles rose a little at this; scant reference is made to the circumspect use of ethnic terms by continental Germanic scholars!

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CHRISTOPHER DYER. *Standards of Living in the Later Middle Ages: Social Change in England, c. 1200-1520*. (Cambridge Medieval Textbooks.) New York: Cambridge University Press. 1989. Pp. xvi, 297.

In this volume Christopher Dyer makes important contributions to the standard of living question by

extending the discussion back to the late medieval period of English history. He begins with an excellent summary of the data collections forming the basis for research and also includes a brief introduction to the historiographical issues raised by the work of other scholars with that data.

In the first chapter the author defines and describes the social classes from the two periods being compared—the thirteenth century and the fifteenth. These are nicely summarized in a convenient chart. Dyer includes estimates of the number of people in each social category and, within those classes, estimates the numbers of individuals enjoying various levels of income. Throughout the chapters on the aristocracy, peasantry, urban dwellers, and wage earners, he introduces the reader to the wide array of primary sources and secondary works on which his discussion is based. His footnotes cite studies based on sources as varied as lay and ecclesiastical account rolls, household accounts, manorial court rolls, archaeological excavations, tax lists, and inventories.

The author's intentions are clearly stated: "My purpose is to define the different levels of living standards in late medieval society, and to explore changes over three centuries" (p. 9). In doing so, he examines evidence of consumption patterns as well as production. In one of the book's strongest chapters, Dyer analyzes the aristocracy as consumers in a careful empirical study incorporating numerous useful tables and architectural drawings.

As a scholar who has focused much of his career on the study of medieval English peasants, Dyer dedicates his longest chapters to them. He approaches the problem of peasant living standards from two angles. First, he builds a model of a peasant budget including income and expenses. Second, he examines peasant "material conditions" by exploring evidence of housing, clothing, and food supplies. Much less attention is paid to urban areas. Fewer secondary works are called on, and the author's interest obviously lies primarily in the countryside. When he does turn to the cities and towns, his interest is in individual living standards rather than in charting the chronology of urban prosperity and decay.

Final chapters on poverty and charity and on weather changes and years of bad harvest complete the study. Dyer finds that only a small percentage of monastic and episcopal incomes was devoted to alms (as little as 2 to 5 percent). He points to the importance of "hidden sources" of economic support. Neighbors and family members probably contributed more to the support of the poor than did large institutions. Dyer's main conclusion is that there was an overall improvement in living standards by the fifteenth century. He makes the important point that there was a virtual absence of mortality associated with bad harvests between 1375 and 1520; "the horrors of 1315–1318 would never be repeated" (p. 277).

This book raises important broader issues. Com-

mon parlance assumes a simple definition for the concept "standard of living." There is, however, debate among current economists, philosophers, anthropologists, and modern historians about the meaning of that term. Dyer has here invited medievalists to join this important discussion, even though he himself engages only sparingly in issues of definition. For example, his definition of poverty is "life threatening deprivation" (p. 234). It is then not surprising that the author is forced to state that "the survival of the medieval poor still remains something of a mystery" (p. 257).

The economist Amartya Sen has criticized the use of utility (pleasure, desire, choice) as a means of evaluating standards of living and prefers the concepts "capabilities" and "functionings." Dyer quite clearly chooses to stick to the utilitarian approach, while admitting that "assessments of living standards are bound to involve a good deal of subjective judgement, and are inseparable from considerations of the quality of life" (p. 275).

Dyer has thus employed a measuring stick much more easily applied to modern economies than to medieval ones. But this book has made a major contribution to discussions of the criteria that should be used to measure economies of the preindustrial past. Can scholars be expected to determine in any meaningful way the standard of living of medieval men and women? Because we do not yet know how to characterize the medieval English economy by categories now used to describe developed and developing economies, the goal this author set for himself—defining levels of living standards—is very difficult to achieve. Dyer never states that his intentions are to compare medieval standards of living with our own, but any attempt to "define levels of living standards" inevitably invites such challenges. Historians cannot evade the problems of cross-cultural comparisons when discussing this particular topic.

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PHILIP MORGAN. *War and Society in Medieval Cheshire, 1277–1403*. (The Chetham Society for the Publication of Remains Historical and Literary Connected with the Palatine Counties of Lancaster and Chester, third series, number 34.) Manchester: Manchester University Press for the Chetham Society; distributed by St. Martin's, New York. 1987. Pp. 254. \$48.50.

In this impressively researched monograph, Philip Morgan explores the interrelationship between "military service and the fabric of local society" in Cheshire from the beginning of Edward I's Welsh campaigns to the defeat of Henry Percy at the battle of Shrewsbury. Morgan traces the evolution of military obligations and service and clarifies the changing character of Cheshire's military community. The pic-

ture that emerges is one of a dynamic and complex society in which local tenurial, economic, and personal considerations combined with the needs and policies of the county's earls—most notably the Black Prince and Richard II—to shape and reshape patterns of military recruitment and of gentry affiliations. Given its title, one might expect this book to fit in most comfortably with the works of H. J. Hewitt and Michael Prestwich. But it has at least as much in common with Nigel Saul's and Michael Bennett's studies of community in medieval Gloucestershire and Cheshire. And, in its focus on lordship, patronage, and affinities, it explores on the county level the same issues that Chris Given-Wilson (*The Royal Household and the King's Affinity* [1986]) and J. M. W. Bean (*From Lord to Patron* [1989]) consider from a royal and nation-wide perspective.

As a marcher lordship, medieval Cheshire had "a military responsibility woven into the fabric of tenurial relationships to a greater degree than in the rest of England" (p. 2). Intermittent warfare with the Welsh kept military service a critical obligation of tenure long after it had ceased to be in more pacific counties. The independence of the earldom, moreover, was guaranteed only by the goodwill and interest of the king; the commitment of Cheshire men to warfare was the price they paid for the liberties of the county.

Changes in the technology and practice of war in the late thirteenth and fourteenth centuries, notably the replacement of the poorly armed footsoldier with the hobelar and mounted archer, made warfare into the preserve of the well-to-do. With few significant monastic estates, no resident nobility, and successive minorities of royal earls, the county's leadership in both war and peace devolved, by the mid-fourteenth century, on the gentry, who formed overlapping affiliations that competed, often violently, for the county's limited resources, especially for the earl's patronage. These "affinities" shaped the patterns of military service in the county.

The ambitions and needs of Edward, the Black Prince, also revolutionized military recruitment and organization in Cheshire, as the gentry "felt the full impact of war which grew from the claims of a masterful lordship exercised by the Black Prince" (p. 139). Although the county's gentry had long enjoyed a reputation for being warlike, it was only under the prince that Cheshire became fully militarized. Military service was based no longer on communal obligations but on the institution of indentured retinues through which the prince secured the loyalty of the gentry affinities. The defense of the Principality of Aquitaine not only made new and extraordinary financial and manpower demands but also created, in its emphasis on garrison duty and long-term service, a new military community composed of lesser gentry and younger sons who made war their profession. These men formed a specialized "military community" that came to dominate the military organization of the county at war. The more

well established gentry, on the other hand, fought on only a few occasions, taking care not to be away so long as to neglect their family connections, offices, and estates.

Morgan concludes his book with a study of Richard II's reliance on the military muscle of the Cheshire gentry and Richard's and Henry IV's practice of "the politics of faction and exclusion." This is the least satisfying part of the book. The subject has been studied intensively, and Morgan adds little that is new. After spending a chapter and a half describing the emergence of a specialized "military community" within the gentry, Morgan virtually ignores the existence of these professional soldiers in his discussion of the events of 1387 to 1403. Even the tensions and rivalries among the local gentry affinities are not given the attention that one would expect.

Although his prose is opaque, Morgan's analysis is lucid and persuasive throughout. But what is one to make of his findings? Cheshire, as he himself points out, was militarily, administratively, and ecclesiastically distinct from the rest of England. The county's unique character and conditions make it difficult to generalize beyond its borders. Nevertheless, the author is to be congratulated for having produced a first-rate piece of local history and for accomplishing his stated aim of writing "une sociologie de la guerre" for medieval Cheshire that fully satisfies Contamine's criteria.

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LOTHAR KOLMER. *Promissorische Eide im Mittelalter*. (Regensburger Historische Forschungen, number 12.) Kallmünz: Michael Lassleben. 1989. Pp. 371. DM 45.

This study of promissory oaths, originally the author's *Habilitationsschrift* of 1986, treats a great variety of solemn promises made to provide assurance to others, including those of vassalage, guild membership, religious confraternities, and the Peace of God. In spite of the rather sweeping title, Lothar Kolmer focuses especially on Germany between the seventh or eighth century and the twelfth century. His bibliographies of primary sources and secondary works, covering thirty pages of small type, suggest the scope of his enterprise: he includes most of the Germanic chroniclers of the early and High Middle Ages, some councils and letters, a number of *vitae*, and a very large selection of modern German scholarship.

But all of this erudition seems oddly unfocused. Highly structured chapters, each divided and subdivided into numbered sections, present a great number of examples, but the organization is never made clear. The book has no introduction, and chapter 1, "Promissory Oaths in Literature," plunges immediately into oaths in the plays of Aristophanes and in the *Nibelungenlied*; oaths are not even defined until chapter 4. The other chapters cover oaths as both

hierarchical and horizontal bonds, the forms of oaths, when oaths were permitted or forbidden, and dispensation or punishment for breaking oaths. Although the author states that he only discusses in detail a selection of the some two thousand oaths he found in his sources (p. 70), on many pages the footnotes cover more space than the text. In none of this is there any sense of development of the forms or functions of these oaths over the five centuries that Kolmer studies.

This book will provide a mine of useful information to subsequent scholars working on various forms of oaths, but it lacks a clear thesis or argument of its own. Kolmer concludes that the Middle Ages was the great age of oaths and that oaths went out of use in the eighteenth century because of a weakening of religious impulse (p. 362), but this point is never developed. The principal assertion he makes about the oaths that he studies is that they cannot be clearly categorized or pressed into a single schema. Considering that the topics he covers range from the symbolic importance of hands, to the theological question of whether Jews could swear on relics, to conspiracy oaths, and to weddings, this is not surprising. But Kolmer still thought it appropriate to try to cover them all in a work that, in consequence, becomes little more than an annotated catalog of medieval oaths.

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DIETER STIEVERMANN. *Landesherrschaft und Klosterwesen in spätmittelalterlichen Württemberg*. Sigmaringen: Jan Thorbecke. 1989. Pp. 336. DM 98.

Controlling monastic institutions always played a role in the development of lordship in the medieval period, but the late Middle Ages saw a shift from the tradition of monasteries as patronage pools or sources for occasional subsidies to a situation where transfers of property, even the obliteration of the house, became conceivable. The scaffolding for the Reformation was being erected well in advance of 1517. Württemberg is a concrete example of exploitation of religious houses as a means to territorial sovereignty. The sources of lay control of religious houses included *Vogtei* (advocacy), deriving from the rights of a founder, and the associated institution of *Schirm* (protection), deriving originally from election of a protector from the lay nobility by houses needing military and judicial assistance.

The princes of Württemberg approached religious communities from a strong position as papal allies in the struggle with the Hohenstaufens. Although the territory was located in the heart of Swabia and honeycombed with imperial estates, the counts' naming as imperial advocates in lower Swabia in 1298 made it hard for religious houses to escape by claiming the status of imperial abbeys. Because a high proportion of the religious houses obtained by Würt-

temberg arrived as already subordinated units in lordships obtained by inheritance or purchase, direct seizure of religious houses was moot.

Reform of religious institutions was an old tactic that was used most often in the fifteenth century for the reorganization of Dominican nunneries. The Brethren of the Common Life, who profited from transfers of property from existing institutions under Eberhard im Bart, would lose their property in Württemberg with papal approval in 1516. The most dramatic example of expropriation of ecclesiastical property for new functions came in 1477 when the collegial foundation of Sindelfingen, long exploited piecemeal, became an endowment for the new University of Tübingen. Oddly enough, although the Reformation would abolish communal religious life, units of property represented by religious houses would persist into the nineteenth century.

Most religious houses in Württemberg had ceased to be hotbeds of spiritual life by 1400, and in this study they were counterpoint to the emergence of territorial religious policy. Jurists viewed religious life in the principality as a functional unity, not as a complex of inviolable rights and privileges. By the dawn of the sixteenth century, welfare institutions were already in the hands of lay authorities. Traditional clerical immunities to routine taxation did not protect them from war levies. Partly as a result of the prince's policies, non-noble monks and nuns became common, and the *Erbarkeit*, the amalgam of lower nobles and bourgeois supporting the development of princely government, was deeply nested in what was increasingly a state church. In the end, this was a process of state making (*Verstaatlichung*).

Dieter Stievermann's study bears the marks of a German *Habilitation* thesis: it is systematic literally to a fault, so that any narrative appears only after definitions of terms and sketches of legal problems. The result is that some excellent information on pre-Reformation Württemberg emerges only on second reading.

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CHARLES HIGOUNET. *Les Allemands en Europe centrale et orientale au Moyen Age*. (Collection historique.) Paris: Aubier. 1989. Pp. 454. 195 fr.

Charles Higounet's study is the first general account of German eastward migration since the publication of Rudolf Kötzschke and Wolfgang Ebert's *Geschichte der ostdeutsche Kolonisation* (1937), which was inevitably a product of its time. Its appearance is especially welcome now when Eastern Europe is once again undergoing a fundamental transformation. A German edition of this book, *Die deutsche Ostsiedlung im Mittelalter*, already appeared in 1986. The late Charles Higounet, who first became interested in this



topic as a prisoner of war in Silesia, rightly calls the so-called *Drang nach Osten* one of the most controversial subjects in history. The German title offers a better insight into the book's content. It is an account of the geographical spread of German settlement, both rural and urban; it does not deal with the internal history of the various states or with the Hanseatic League. Higounet insists that German colonization should be seen not in anachronistic, nationalistic terms but rather as part of West European outward expansion during the High Middle Ages. The result was the creation of a distinct culture, symbolized by the brick Gothic churches of the Baltic.

The first section is a general history of Germany's relations with its eastern neighbors from the sixth century to 1200. The second part, the heart of the book, is a detailed, chapter-by-chapter description of the spread of German villages and towns in specific regions: Holstein and Lauenburg; the area between the Saale and Elbe; Brandenburg and its marches; Mecklenburg and Pomerania; Austria and the eastern Alps; Bohemia and Moravia; Silesia; Hungary, Slovakia, and Transylvania; Poland; Livonia; and East Prussia. The third part deals briefly with a variety of topics such as *locatores*; rural and urban topography; farming techniques; municipal government; the Jews; the mendicant orders; the diffusion of the German language; and universities. Higounet discusses in a brief epilogue the causes for the German retreat after 1350. This is a balanced and informative introduction to a difficult and important subject that could perhaps have been written only by a neutral outsider. I do have, however, a number of reservations about the book.

An introductory note indicates that all place names are given in German with the present-day name in parentheses. In fact, such forms as Breslau and Wrocław appear throughout the book without reference to the other, even in the same sentence (p. 184), and in the index with different page citations. To add to the confusion, many names are misspelled. I found the following mistakes on one page (p. 167): *Cissi* (instead of Cilli), *Klagenfurt* (Klagenfurt), *Radkersburg* (Radkersburg), and *Brück-am-der-Mur* (Bruck an der Mur). Perhaps this is the publisher's fault. There are a few strange factual errors. St. Adalbert of Prague, who was martyred in Prussia in 997, is said to have been killed by the Ruthenians in 977 (p. 73). Vienna is alleged to have been enfeoffed first (when?) to an Aribio of Salzburg (p. 164). I never knew that there was an archbishop named Aribio, let alone that Vienna belonged to Salzburg.

There is no bibliography because Higounet believed that the footnotes would provide a sufficient guide to the literature. Regrettably, there are numerous bibliographical omissions. He did not cite, for example, the *Conversio Bagoariorum et Carantanorum* or the *Chronicon* of Thietmar of Merseburg and Herwig Wolfram's or Helmut Lippelt's studies of these crucial sources; Imre Boba's work on Moravia; Karl Leyser

on Ottonian Saxony; Hans-Joachim Freytag or Gerd Althoff on the Billungs; Harald Schieckel on the Meissen ministerialage; Hans Pirchegger on the *Untersteiermark*; William Urban on the Baltic crusades; or my work on the friars. In short, this is a valuable but strangely flawed book.

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RICHARD C. HOFFMANN. *Land, Liberties, and Lordship in a Late Medieval Countryside: Agrarian Structures and Change in the Duchy of Wrocław*. (Middle Ages Series.) Philadelphia: University of Pennsylvania Press. 1989. Pp. xx, 578. \$49.95.

Richard C. Hoffmann's study of agrarian structure and change in the duchy of Wrocław in the late Middle Ages fills an important gap in our knowledge, especially since so little has been written in English concerning economic development in Eastern Europe. Yet in many respects the story is a familiar one. From the late twelfth century, population grew, the area under the plough increased, and German law spread. Moreover, since the duchy escaped the onslaught of the Black Death, this demographic and economic expansion continued until the area was hit by the epidemics and famines of the 1360s and 1370s. Thereafter the inhabitants enjoyed a fifty-year period of equilibrium with the land producing enough to support both the demands of its lords and the needs of townspeople and peasants. But in the mid-fifteenth century, erratic and hazardous weather, monetary chaos, and the violence spawned by the Hussite revolution all combined to destroy the old forms of society. Peasants died and were not replaced. They left their threatened farms and did not return, and they defaulted on their traditional obligations to their lords. When the economic situation improved, many lords increased their authority and, during the course of the sixteenth century, were able to impose new dues and burdens on their peasants such as entry fines and labor services.

In building his picture, Hoffmann relies heavily on legal compilations, charters, and survey documents. These allow him to delineate broad changes over time but do not permit detailed discussion of agricultural operations. He points out, for example, that, by 1425, 40 percent of the demesne farms that had existed a century before had either shrunk or been converted into tenant land and that arable demesnes were more likely to be converted than stock farms. But with few surviving demesne accounts, he cannot discuss the relative productivity of arable and pastoral farming. No information was available on grain yields, the use of legumes, the rate of seeding, or the ratio of livestock to cultivated acreage. Thus, when, in the sixteenth century, lords exercised greater economic supervision of their estates, it is not clear whether, in



fact, they produced more grain and stock than in preceding centuries.

One of the most interesting questions raised by this book is why the imposition of servile burdens on the peasantry met with so little resistance. Hoffmann explains that the *schulzen* (village notables) who, in the fourteenth century, had served as leaders in their villages, and as mediators between peasant and lord in the fifteenth century, lost those roles when the hereditary officeholder was bought out and replaced by temporary appointees, subservient to the lord. Thus, peasant communities, with their historic leadership cowed or departed, were unable to defend themselves against the new strength and drive of their lords (p. 358). But such an answer is really only a partial explanation. One wants to know why the *schulzen* were willing to sell or give up their rights and why, in their absence, other leadership did not emerge. In this context it is somewhat surprising that Hoffmann makes no reference to the ideas of Robert Brenner, who suggests, for example, that because in Eastern Europe peasants in the twelfth and thirteenth centuries were often granted rights and good conditions, they had no tradition or history of struggle and successful resistance on which they could draw when lords changed their policy in the direction of greater exaction and control. Even if Hoffmann does not agree with Brenner, it would have been interesting to learn his opinions on the subject, in view of the continued vitality of the Brenner debate.

Although this book provides a stronger picture of what happened than why it happened, it is very solidly researched, with a keen appreciation of the constraints imposed by the geographical environment. Most scholars could learn from it.

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ALAN HARVEY. *Economic Expansion in the Byzantine Empire, 900–1200*. New York: Cambridge University Press. 1989. Pp. xvi, 298. \$49.50.

Any student of Byzantine history must be struck by the suddenness of the empire's collapse before the Seljuk Turks in 1071 and the Fourth Crusade of 1204. Prevailing opinion used to trace the trouble to the early tenth century, when many small holders who had previously supported the empire's economy and army started to sell their land, becoming tenants on the buyers' large estates. This "feudalization" of Byzantium allegedly left the empire impoverished and weak. Over the last twenty years, several scholars led by Michael Hendy have shattered this consensus by assembling evidence of increasing Byzantine prosperity right up to 1204. They have noted that the formation of large estates need not be a cause or even an effect of general impoverishment and have attributed the weakness of the government only to a failure to tap the wealth of its great magnates. No one has

made a convincing case against this argument, which is gradually and deservedly winning general acceptance.

Alan Harvey, who numbers Hendy among his teachers, has now written the first book specifically focused on Byzantine economic expansion between 900 and 1200. A revision of his doctoral dissertation, the book includes some useful discussions; the accounts of how monetary fluctuations affected taxation (pp. 90–102), of the various demands on taxpayers (pp. 102–14), and of the Byzantine diet (pp. 164–82) are detailed and clear. Although the discussion of demographic growth (pp. 47–79) relies rather lopsidedly on Macedonian evidence, such growth plainly did occur and cause economic expansion. But the author is most interested in formulating another hypothetical cause.

According to Harvey, "The Byzantine social formation consisted overwhelmingly of peasant producers" who, because of "a distaste for manual labor," would have produced only what they needed for mere subsistence had not "political coercion" forced them to produce a surplus for the state and landowners (pp. 1–2). Between 900 and 1200 "a greater part of the peasantry was subordinated to large landowners," and so "the cash demands on the peasantry were increasing, providing the state and feudal landowners with greater revenues" (p. 263). Thus, feudalization spurred economic expansion by requiring peasants to produce more to satisfy their landlords.

Harvey's thesis, incongruously applying Marxist analysis to the non-Marxist ideas of Hendy and the Western medievalist Georges Duby, appears in the introduction and conclusion but is not supported by the intervening chapters, which do little more than describe the expansion. Harvey asserts rather than argues his premises, then uses them to dismiss views based on different premises. When Paul Lemerle sees Byzantine society "in terms of two different status groups rather than economic classes," Harvey observes, "the problem with this approach is that it analyzes the social structure of Byzantium in terms set down by the Byzantines themselves rather than those of the modern historian" (pp. 41–42). Harvey also rejects John Teall's remark that a Byzantine estate was a "capital investment designed to produce returns" by asserting that landholders aimed only at "capital formation within the limited perspective of feudal landownership" (pp. 161–62).

Harvey rejects Byzantine sources for Byzantine thinking in favor of a concept of feudalism derived from Western medieval history. As he fails to relate his concept to his evidence, he cannot plausibly use it to show that Byzantine peasants worked harder for landlords than they did for themselves. In the end his theorizing serves mainly to distort what would otherwise be a solid, if mostly descriptive, study.

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EVA DE VRIES-VAN DER VELDEN. *L'Élite byzantine devant l'avance turque à l'époque de la guerre civile de 1341 à 1354*. Amsterdam: J. C. Gieben. 1989. Pp. 296.

Provocative and stimulating, if not ingratiating, this book follows in quick succession Eva de Vries-van der Velden's publication of her doctoral dissertation, aimed at a (highly negative) reappraisal of the Byzantine scholar-politician Theodore Metochites. Despite the title of this work, it is not a systematic social history nor a coherent analysis of the tragic period in which John Kantakouzenos's usurpation confirmed the inevitability of the Byzantine empire's ultimate conquest by the Turks. It is rather a set of essays serving as contributions toward such a study. It is also a caustic critique both of the original sources and of modern scholarship, aimed at setting the record straight by the author's lights, no holds barred.

The first of the book's two parts consists of a prologue and two chapters that discuss the character and evaluate interpretations of Turkish and Byzantine history in the first half of the fourteenth century, as general context. The author excoriates recent Turcological scholarship, condemning efforts to mitigate the savagery of the Ottoman conquests or to portray a Turkish "assimilation." She also takes aim at a number of clichés common to treatments of the Palaiologan era. Challenging the concepts of the aristocracy as "military" and dismissing portrayals of Byzantine "feudalism," she also rejects the assumption (hardly the first to do so) that the aristocracy uniformly supported Kantakouzenos as one of its own. But although pointing out what the Byzantine elite was not, she does not offer much of a picture of what it was.

The second part is composed of individual discussions of six personalities of the era whose writings represent our most important sources and who are discussed (in no logical order) for the differing understandings that they represent of the Turkish menace. The book ends abruptly, with no synthesis of conclusions or any effort to project them onto a larger perspective to illuminate late Byzantine society. Presumably, the impressionistic individual portraits are to speak for themselves.

The meat of the book is thus in the author's individual reinterpretations of each of her six personalities, on the basis of their writings. Kantakouzenos himself is the author's villain, whose consuming ambition overrode any perspective on the consequences of his actions, the record of which he falsified in his historical account. Nikephoros Gregoras is a psychological caricature, his political allegiance complicated by his compulsive hatreds for the rebellious populace and for the hesychast doctrine. Gregory Palamas is a fanatic monk, a theological sham, a self-centered partisan of the rich, contemptuous of the humble, an inconsistent partisan of Kantakouzenos, aching for

the patriarchal rank that eluded him, and, in his famous letter on his captivity among the Turks, merely self-serving.

Demetrios Kydones is portrayed as an antipopulist intellectual, equally passionate as pro-Kantakouzenian and anti-Palamite, a convert not only to Roman Catholicism but also to a belated recognition of the truth of the Turkish menace and even to some scant sympathy for popular suffering, even as he became totally alienated from his countrymen. Metropolitan Matthew of Ephesus, a contemptible, self-serving ecclesiastical politician at least represents an eyewitness to the Turkish conquest and a prototype of the Greek prelate under Ottoman rule. Only Alexios Makrembolites among these personalities (and just barely a member of the "elite" himself) could appreciate the poor, but even his perception is defective, since his writings, in the author's words, "font penser aux tirades d'un évangéliste populaire de nos jours" (p. 252).

The author leaves us with a picture of an "elite" consisting of no more than knaves or blockheads, making us wonder if she finds anything to admire or enjoy in the material she treats. The only sympathies that she expresses are for some abstraction identifiable as ordinary people—that is, the ones she is not studying and who (like the Zealots of Thessaloniki) hardly ever appear. Scathing is her contempt for most modern scholars: hardly any cited escape without savaging for one or another failure of understanding, while many are dismissed as all but willfully misleading. Nonideological in her prejudices, she is equally contemptuous of Marxist interpreters and of those who take seriously the theological issues and ideas of the fourteenth century—the leading modern scholar on Palamism being casually labelled as "le bigot Meyendorff" (p. 157, n. 15).

The author actually has much worthwhile to offer. She is a tireless reader of very difficult source texts and a perceptive interpreter of them, and she commands the secondary literature with confidence. Despite the bull-in-the-china-shop tone, her pitilessly critical judgments often represent refreshing and challenging perspectives. Subsequent scholarship will have to reckon with her critiques of each of her six personalities: she makes particularly telling points on Kydones, and, although her differences with Ševčenko will require scrutiny, she makes a real contribution to understanding Makrembolites, augmenting our repertoire of his texts by appending her annotated edition of segments from his important but previously unpublished "Second Discourse."

In the future, the author may learn to control her spleen and go beyond merely tearing things down so as to build something positive in their place. For now, her new book is an important and worthwhile contribution, even if it seems sometimes more like a terror-

ist raid than a coherent venture in responsible scholarship.

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#### MODERN EUROPE

RENATE BLUMENFELD-KOSINSKI. *Not of Woman Born: Representations of Caesarean Birth in Medieval and Renaissance Culture*. Ithaca: Cornell University Press. Pp. x, 204. \$27.95.

Birth and death are the two most dramatic moments of life. Until the past few centuries, these two events converged at a Caesarean delivery, when a baby would be cut from the womb of a dead mother. "Not of woman born" because he or she was the child of a corpse, the newborn would often live just long enough to be baptized. By examining this emotionally charged event in iconography and medical literature, Renate Blumenfeld-Kosinski explores medieval and Renaissance mentality and writes a history of professional rivalry and the persecution of women.

Focusing on images of Caesarean birth, Blumenfeld-Kosinski exploits a particularly fruitful historical source. These illustrations are found, for the most part, in manuscripts of the Bible and historical texts, not in medical books, for obstetrics played an insignificant role in the university curriculum and surgical education. Unlike other illuminations in these codices of events such as coronations, battles, or even normal childbirth, scenes of Caesarean delivery do not follow an iconographic tradition, for none existed. The author makes clear to us that illustrators had to gather information from contemporary witnesses and participants in order to know what to depict, and thus these images are more revealing about medieval life and society than are other conventional scenes.

One of the most important tales told by Caesarean pictures is the story of the marginalization of women in obstetrics. Before 1400, midwives are found center stage, demonstrating teamwork, decisive action, and surgical skills. After this date, when Caesareans on living women seemed at least a possibility, male surgeons began to take over, especially in France. Blumenthal-Kosinski's point that males entered obstetrics in the fifteenth century via the Caesarean operation is a significant one, contradicting the standard account that, in the sixteenth and seventeenth centuries, birthing instruments such as crotchets and forceps provided the decisive technology for this takeover.

Both the medical establishment and the church worked energetically to discredit and marginalize female healers. For most of the Latin Middle Ages, women held a monopoly in the birth chamber, although this had not been true in the antique and Arabic traditions. From the late thirteenth century on, however, following the rise of the university and

the establishment of medical faculties, a new self-definition of physicians and surgeons, and the eventual entry of this second group into the academic corporation, led males to reclaim their ancient functions in birth by wresting them away from women.

Blumenthal-Kosinski ties in this development with the witchcraft persecution and brings a fresh perspective to the oft-repeated connection made between the midwife and the witch. Carefully rereading the standard inquisitorial treatise on witchcraft, the fifteenth-century *Malleus Maleficarum*, she demonstrates that two different types of midwives figured in the document and thus in the eyes of the persecutors. The first of these, the good one, in charge of procreation and the protection of pregnant women, worked against the second, evil enemy of reproduction, who rendered men impotent and practiced contraception, abortion, and infanticide. This latter group, the personification of woman's dark powers and her control over sexuality, had to be removed from the position of responsibility that these midwives held over the child's life and baptism during a Caesarean birth.

This book presents a fresh, well-worked-out interpretation of the role of women in Caesarean birth and, by extension, medieval medicine. Particularly strong is the author's iconographical analysis, which illuminates the often-blurry plates reproduced by Cornell University Press. In developing her argument, Blumenthal-Kosinski relies heavily on secondary accounts and other scholars' translations and only minimally on her own use of manuscript sources. But her interpretation of the material, especially of manuscript illumination, is very well done, and this book represents an important contribution to the history of medicine and of medieval women.

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HEIKO A. OBERMAN. *Luther: Man between God and the Devil*. Translated by EILEEN WALLISER-SCHWARZBART. New Haven: Yale University Press. 1989. Pp. xx, 380. \$29.95.

The publisher's dust-jacket claim that this book is "the definitive biography" of Martin Luther so lacks credibility that the author himself may wish to disown it. The work is in fact neither a biography nor "definitive," if only because its disregard for chronology fosters the impression that Luther's life did not unfold with one episode following on another.

The twin theses of this collection of interrelated essays are that Luther stood not just between God and the devil but also between the Middle Ages and the Enlightenment. Together, the various chapters touch down here and there on the topography of Luther's career with all the intensity—and unpredictability—of a tornado. Thus, a prologue and two chapters on the Reformation as German and medieval events

include tantalizing comments about the three central treatises of 1520 and precede a sustained discussion of Luther's childhood. The section that treats his development as a professor of theology, 1513–18, inexplicably concludes with a disquisition about his lectures on Genesis, 1535–45. In the same manner, a chapter on the crucial years from 1517 to 1521 begins with a brief discussion of the *Large Catechism* (1529), moves backward to Luther's early relations with Johannes Staupitz (pre-1513), forward to spring 1521, back to fall 1518, back again to fall 1517, forward again to spring and fall 1518, then forward once more to 1521.

Such chronological chaos does not allow full coverage of the subject's career. Of Luther's *On the Bondage of the Will* (1525), Heiko A. Oberman declares that "were these the only pages of his writings to have survived, we could deduce from them the total scope of his thinking" (p. 212). True to his word, the author touches on the Sacramentarian Controversy, Luther's family life, his anti-Jewish writings, and his death but virtually ends the story in 1525, with two decades yet to pass of what was becoming an ever more public career. There is thus little to be learned from these pages about Luther's involvement in practical evangelical politics, his later work as *defensor fidei*, or his labors to create a distinct church complete with an educated clergy.

Instead, Oberman repeatedly pictures Luther as an "existential" theologian, who was caught between God and the devil, and declares that this "'narrow perspective' opens us to the total vista of the Reformation" (p. xix). Most historians would disagree with both this statement and this approach.

The author's point of view is, however, understandable, because he is addressing not historians first and foremost but contemporary, near-contemporary, and ecumenically minded church historians and theologians. He confronts them with Luther in his "totality" (p. xix) as a man who took the devil seriously. Here one can only ask whether this Luther between God and the devil is really the total historical Luther. Granted that he found the devil keenly real, where is the Luther who set to music the triumphant "A Word Will Quickly Slay Him"?

This book sketches one side of Luther and one that the twentieth century has perhaps taken for granted. It also presents a fair reading of those documents that the author has chosen. Whoever knows Luther well will find in Oberman's work many penetrating insights and much that will require thought. Whoever does not know Luther well should turn initially to other works for history and biography and to this book only thereafter.

JAMES M. KITTELSON  
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LOUIS CHATELLIER. *The Europe of the Devout: The Catholic Reformation and the Formation of a New Society.*

Translated by JEAN BIRRELL. Cambridge: Cambridge University Press or Editions de la Maison des Sciences de l'Homme, Paris. 1989. Pp. xiv, 270.

Despite its subtitle, this book is a study not of the Counter Reformation but of a subject at once topically more specific and of greater chronological sweep. The focus is on the Marian congregations established by Jesuits in the sixteenth century for the purpose of teaching laymen how to practice their Catholic faith and lead Christian lives. But the ultimate objective of these organizations, according to Louis Chatellier, was nothing less than to reform the world. He poses two questions: "Did they succeed in this aim? Was a Catholic society gradually constituted in the years, or rather centuries, following the Council of Trent" (p. xi)? This book answers both questions in the affirmative.

Working as a sociologist of religion skilled in the use of quantification, Chatellier builds his case on extensive primary materials from the archives of Marian congregations in parts of Belgium, the Rhineland, Bavaria, Austria, Rome, and Naples. His discussion is presented in a series of clear and elegantly structured chapters that trace the development of the congregations from their foundation to the end of the Old Regime, their transformation in the nineteenth century, and their impact on the religious geography of the twentieth.

The thesis of the book is that the Marian congregations were extremely successful in drawing to themselves men from all strata of society, first in an urban and then a rural setting, and in creating a sense of brotherhood among significant numbers of Catholics. Beyond offering each other moral support, congregation members were effective apostles of their faith for men belonging to ever wider circles of the population and their families. The *dévots* of the seventeenth century helped the congregations adapt to changes in society and broaden their scope from the emphasis on collective exercises of religion to concern with the social welfare of the *sodales*, the formation of a reformed clergy, and the permeation of Catholic teaching everywhere.

The author believes that Marian congregations were the ferment of a new society. Resolutely rejecting the idea of large-scale dechristianization in the eighteenth century, he argues that the network of confraternities helped maintain religious devotion among the Catholic population of Europe. Members went to confession and communion regularly, participated in common prayers, processions, and festivals, and publicly manifested their faith. In enjoining workers to fulfill their duties and the poor to accept their lot, the congregations not only were forces of order but also contributed to the strengthening of bourgeois values. They were cradles of Christian syndicalism in the nineteenth century. Ultimately their influence, albeit in changed and more modern form, explains the continued existence of Catholic



areas in modern Europe, and possibly even of European Christian democratic parties.

The least satisfactory part of the book is the last where the leaps from early modern congregations to modern political parties are made too rapidly. The most valuable sections present a careful discussion of the role that Marian congregations played in maintaining and spreading Catholicism from the sixteenth to the eighteenth century. The author is sensitive to their effect on women, workers, and the poor. He has offered the reader a sustained and forceful argument, inviting further discussion of an important aspect of the Counter Reformation. This book definitely deserves notice.

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YOSEF KAPLAN. *From Christianity to Judaism: The Story of Isaac Orobio de Castro*. Translated by RAPHAEL LOEWE. (Littman Library of Jewish Civilization.) New York: Oxford University Press, for the Littman Library. 1989. Pp. xv, 531. \$86.00.

In this biography, Yosef Kaplan presents his subject, the seventeenth-century physician and religious thinker Isaac Orobio de Castro, as a mirror of mainstream attitudes among those Spanish and Portuguese crypto-Jews who fled the peninsula and settled as Jews in Western Europe. He argues that such well-known and well-researched figures as Baruch Spinoza and Juan de Prado, for whom the struggle to reconcile conflicting cultural and religious viewpoints led to alienation from Jewish life, are the exception and not the rule. This seems the justification for devoting a scholarly monograph to a figure who is clearly neither an original thinker nor a towering personality.

With this corrective end in view, Kaplan guides the reader through the diverse environments, social and intellectual, through which Orobio passed. He traces Orobio's vicissitudes from his birth in Braganza, Portugal, to his years at the universities of Osuna and Alcalá de Henares, to his imprisonment by the Seville inquisition, to Toulouse, and finally to Amsterdam, where he pursued a medical practice and engaged in polemics with Christian theologians and nonconformist Jews. Along the way, the author marshals an impressive array of archival, literary, and scholarly sources. The archival research is especially thorough. The abundance of incidental detail, however, is an impediment to focused reading.

The most valuable chapters of the book are those dealing with Orobio's polemic and apologetic activity in Amsterdam. Orobio's positions are meticulously delineated in relation to the theological and philosophical trends with which he came into contact. Particularly for the reader with some knowledge of baroque intellectual trends, the discussion of Orobio's

cultural milieu and his responses to its various stimuli is rewarding and suggestive.

The author intends the book to be both biography and collective history; however, the book is satisfying as neither. As the narrative of Orobio's life proceeds, one waits in vain for the emergence of a vivid personality. Perhaps this is in part because of the nature of the available material. But the book is also flawed by a lack of genuine psychological insight. In its place we have the clichés typical of so much of converso scholarship: Orobio learned as a child "the secret of how to be a Christian as he walked abroad whilst preserving his Judaism at home and in the inner recesses of his heart" (p. vi); Orobio and his family "threw off the cloak of Christian conformity with which they had for so long concealed their true identity" (p. 107); and so on. Insofar as an attempt is made to sketch Orobio's character, he appears a dutiful, high-minded apologist rather than the "colourful personality" Kaplan avers him to be (p. 304).

As a biography of a figure who epitomizes his society, the book is also problematic. A biography of Orobio would be virtually unthinkable in the absence of the latter's substantial literary legacy. But most of the ex-conversos were not physician-intellectuals and did not write subtly argued polemical tracts. They were merchants and dependents of merchants. It cannot be assumed that Orobio's highly intellectualized way of dealing with cultural and religious conflict reflects the others' views. To be sure, as a man who succeeded in abandoning Catholic practice and integrating himself into Jewish life, Orobio is certainly more representative than his better-known contemporaries Spinoza and Prado. This granted, Kaplan's book still falls short of breaking new ground and is overshadowed by Y. H. Yerushalmi's masterful biography of the Italian physician and ex-converso Isaac Cardoso, a figure to whom Orobio bears an unusual superficial resemblance.

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PETER SAHLINS. *Boundaries: The Making of France and Spain in the Pyrenees*. Berkeley and Los Angeles: University of California Press. 1989. Pp. xxi, 351. \$35.00.

What a good idea! While others talk plenty about the periphery in the transformations of states, Peter Sahlins actually studies how people of the Cerdaña (the Catalan region partly seized by France when it took all of Roussillon in 1659) worked out their national identities between then and the drawing of a precise French-Spanish boundary in 1868. The word "identities" belongs in the plural: throughout the process people on the spot flexibly identified themselves as French, Spanish, or Catalan, depending on context and advantage. Sahlins not only reconstructs



this experience from an ethnographic perspective, giving vivid views of local affairs, but also offers insights into French and Spanish strategies of state formation. On the side of identity, Sahlins shows how sets of people formed shared identities in opposition to each other, in a process of continuous interaction, making each other into "Frenchmen" and "Spaniards" before the rulers of the respective states had a clear idea who belonged to which. Rare is the historian who understands so well two crucial truths about identities: they do not come ready-made from outside, and they reside not in individual mentalities but in dynamic social relations. On the side of state formation, Sahlins shows that the idea and practice of frontier moved from defensive zone to precise geometric line, while the idea and practice of sovereignty moved from jurisdiction over sets of persons to jurisdiction over territory—both critical developments in the formation of today's sharply bounded states. (Even today, however, older practices have left the Spanish-held town of Llívia completely surrounded by French villages.) Early seventeenth-century Cerdàña formed a county of eighty-odd settlements that grouped into four quarters participating in a General Council of Syndics. Royal administration met mountain oligarchy, markets, and religious festivities in the central town of Puigcerdà. The Treaty of the Pyrenees of 1659, however, started a long effort to define a natural frontier and to divide the mountain basin in two. Unsurprisingly, disputes and delineations occurred most frequently when agents of one of the two powers attempted to impose taxes or control smuggling, which amounted to the same thing; more than civilian administration or military control, fiscal incentives led to sharp drawing of lines. Only in the later eighteenth century did the two states, especially France, start insisting on neat boundaries as a matter of national sovereignty.

Identity and state formation interlocked when it came to personal advantage. Many richer families had property on both sides of the border and claimed the residence and nationality that would cost them less taxes. "Spaniards" who bought land in French territory during the later eighteenth century could not only gain exemption as foreigners from the French *capitation* but also enclose their land advantageously, whereas those in Spanish territory could not. The "French" in the villages in which "Spaniards" bought complained bitterly to French royal authorities about this reduction in their tax base. In this and many other ways, the social strategies of local people made the frontier and identities defined by it more and more powerful realities. The states involved also insisted increasingly on distinction by word and deed; the French revolution and the long French-Spanish wars that it began cemented a relatively well defined frontier and made the consequences of belonging on one side or the other dramatically different. That did not, however, keep the people of Cerdàña/Cerdagne/Cerdanya from shifting their identities among

French, Spanish, and Catalan as a function of context and interest.

CHARLES TILLY

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FRANÇOISE WAQUET. *Le Modèle français et l'Italie savante: Conscience de soi et perception de l'autre dans la république des lettres, 1660–1750*. (Collection de l'Ecole française de Rome, number 117.) Rome: Ecole Française de Rome. 1989. Pp. 565.

Françoise Waquet's important book is about the relationship of cultural superiority and inferiority between France and Italy. She uses the history of erudition as a model for the study of intellectual communities. Her work may be regarded as history of science, broadly conceived, built on a solid documentary base of learned journalism, the book trade, and scholarly correspondences. Showing the influence of her mentors Marc Fumaroli and Bruno Neveu, Waquet carefully analyzes well-known and long-lost cultural figures and produces a convincing thesis.

French and Italian scholars mutually reinforced stereotypes of inequality. Italy, whose scholarly luminaries had shone so brightly during the Renaissance, was overshadowed by France in the seventeenth century; in 1670, a Frenchman could say, "L'Italie, c'est rien." Many Italians marveled at the high quality of scholarship in the promised land of Louis XIV's France, "la dotta nazione." Travelers to Paris saw the king's support of the royal library, observatory, botanic garden, and academies. Parisians such as the Maurists of Saint-Germain-des-Prés used excellent facilities for research and publication to produce many volumes. Thus, the world of scholarship came to reflect French grandeur.

Yet Waquet rightly questions historiographical traditions that led to an overestimation of France and to the dogma of Italian decadence in the *Seicento*. Works of synthesis have unfortunately perpetuated these views. Waquet accepts the French contribution to epistemological crisis from Descartes onward and the differing effects of the Counter Reformation in France and Italy. She shows how Italians exhausted their earlier models of scholarship and turned northward for inspiration. France gave them "un idéal de liberté modérée" (p. 200), although French realities were not so wonderful. If we move away from ideological discourse and return to the sources, Waquet justly observes, we discover that Italy was not the "terre d'ignorance et d'obscurantisme" that the French thought (p. 219).

Why then was there a "logic of inequality" in Franco-Italian cultural exchanges? Similar inequalities existed elsewhere: G. W. Leibniz in Germany and Gisbert Cuper in the Dutch Republic also felt disadvantaged vis-à-vis France. Pierre-Daniel Huet and other French provincial *érudits* and scientists expressed ambivalent attitudes toward the domineering

tendencies of Paris, as David Lux has shown. Waquet goes back to the Renaissance, when in response to Italian claims the French argued that their civilization was not dependent on classical culture or its Italian derivatives. In law, linguistics, theology, and politics, the French exalted indigenous traditions. French self-affirmation was joined with an optimistic conception of time and belief in progress; Italians held pessimistic and cyclical views. Italians saw civil wars and the corruption of Spanish rule, the disappearance of Renaissance patrons, and their own idleness as causes of decline. Attempts at restoration by Lodovico Muratori, Apostolo Zeno, and others came too late. The *translatio studii* had been accompanied by a *translatio imperii* across the Alps. Italians placed obstacles in the way of scholarly communication: many became secretive and refused to correspond with heretics or to allow Catholic foreigners access to their libraries. This defensive attitude proved fatal to the position of Italy in the Republic of Letters. The philosophes endorsed the negative judgment of Italy in general, and Waquet finds that French indifference and disdain toward Italian learning have continued to the present day.

Waquet's book will be essential reading for intellectual and social historians. The dates have been chosen with an eye to political developments as well as intellectual trends, so that even diplomatic historians will profit from reading these learned pages.

MAARTEN ULTEE  
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HAIM BURSTIN. *La politica alla prova: Appunti sulla rivoluzione francese*. (Studi e ricerche storiche, number 136.) Milan: Franco Angeli. 1989. Pp. 207. L. 26,000.

CARLA NARDI. *Napoleone e Roma: La politica della Consulta romana*. (Collection de l'Ecole française de Rome. 1989. Pp. x, 225.

Haim Burstin is already the author of a number of studies of revolutionary Paris, including a book on the Faubourg Saint-Marcel published in 1983 by the Société des Études Robespierriennes. Here, as part of a series dedicated to "new voices" in Italian historical scholarship, Burstin offers a collection of nine recent essays, some of them republished from Italian and French journals. All of them are concerned with the radicalization of the Parisian working class in the ephemeral but fascinating phenomenon of "sans-culottism," discussed both as a historiographical problem and as an opportunity for further research.

Burstin has read widely among the contemporary British and American historians. Several essays incorporate, with some success, the insights of a diverse body of scholars that includes Lynn Hunt, Steven Kaplan, J. L. Talmon, and E. P. Thompson, all of whom, Burstin suggests, are too often ignored by French historians of the revolution. Some of the earlier essays in the collection are part of a continuing

project on working-class life in the Faubourg Saint-Marcel and seem rather fragmentary, but the more analytical pieces, notably the very comprehensive historiographical survey called "The Sans-Culottes: A Dossier to Reopen," are full of useful insights. Too often, Burstin argues, historians have "reduced the world of the Parisian working-class to a functional scheme for interpreting the Year II" (p. 153), without examining the long-term process of social, political, and cultural transformation stretching from the Old Regime to the end of the nineteenth century, of which the revolution is only a part.

The first three essays, although less substantial, provide a similarly refreshing synthesis of the Soboul school with the work of Lynn Hunt, in particular on the creation of a revolutionary political culture. Burstin's analysis of an emerging radical consciousness clearly derives not only from the work of French historians of *mentalité* such as Michelle Vovelle and Maurice Agulhon, who are cited, but also from the giants of recent Italian historiography, Antonio Gramsci and Delio Cantimori, who are not.

The final essay, reprinted from the *Annales historiques de la Révolution française* (1983), demonstrates that Burstin is indeed correct in insisting that there are new questions to be asked and materials in the archives to provide answers. Burstin examines the impact of the National Convention's efforts in the fall of 1793 to bring the sections under its control by reducing the number of sectional meetings to two a week and paying the poorer citizens who attended. Basing his study on materials that he found in the departmental archives of the Seine for the section of Panthéon Français, he provides a fascinating glimpse of a self-contained social world confronted with the realities of revolution.

With the exception of the fact that it is also written in Italian, Carla Nardi's study of the Consulta Romana, the extraordinary council that Napoleon established in 1810 to oversee the creation in the Papal States of a modern, centralized, laicized government, is a very different kind of work. The Consulta only lasted for nineteen months, and the majority of its initiatives were ephemeral, yet Nardi argues that their long-term impact on Italy was profound.

This is institutional history, full of useful information on administrative structures created and decrees enacted but short on analysis. It complements the synthetic works by V. E. Giuntella, Jacques Godechot, and others, demonstrating the impact that French ideas and institutions had in the creation of modern Italy.

Nardi's book is well organized and clearly written, although he too often settles for a simple listing of the projects launched by the Consulta. Nonetheless, the lists themselves are impressive. Nardi suggests that the French were often more captivated by the myth of Rome than were the Romans themselves and thus were less likely to be discouraged by the passivity,

ignorance, and hostility that they frequently encountered.

Any notion of political autonomy in the region ended when Napoleon annexed it to the French empire. Three years later, the great experiment came to an abrupt end with his defeat and the restoration of the pope's clerical despotism, accompanied by a widespread popular revulsion against the godless French. Yet the states of the church were eventually absorbed into the modern world, and the structures and institutions of post-1870 Italy were exactly what Napoleon had in mind when he created the Consulta in 1810.

CLARKE GARRETT  
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J. S. FISHMAN. *Diplomacy and Revolution: The London Conference of 1830 and the Belgian Revolt*. Amsterdam: CHEV. 1988. Pp. 240. 59.50 F.

The diplomacy of the Belgian revolution has been frequently investigated and often treated as a portion of works on larger topics. Few scholars, however, have approached the subject as analytically as J. S. Fishman in the study under review. His investigation deals with the London Conference from August of 1830 until May of 1832, when its work was essentially completed. By this time there was final great power agreement to the terms of Belgian independence, although the Netherlands stubbornly refused their recognition until 1839. Within the limited period examined, Fishman carefully delineates all of the perspectives, proposals, counterproposals, intrigues, and painful accommodations that led to the final treaty. The narrative rests on a wide array of primary materials, and the presentation is convincing. The author is superb on the rationales of each power's reaction to the initial stage of the revolt and on the issue of a recourse to arms at specific moments during its progress. The great power reactions indeed were much the same as they had been at the founding of the July Monarchy. There is an excellent treatment of Louis Philippe's retreat on the prospect of his son, the duc de Nemours, becoming the first king of the Belgians. His action was in response to Britain's position rather than to apprehensions about the views of Russia, Prussia, and Austria. Throughout the work there is a pronounced emphasis on French and British policies rather than on those of the Eastern powers. The latter attempted to act in concert during most of the period but did not feel themselves as heavily involved as Britain and France. The result was that the dominant roles at the conference were played by Palmerston and Talleyrand, while the Eastern powers simply wanted to see the crisis ended and became progressively more impatient with William I whose obstinance and virtual defiance of the conference made its work more difficult and delayed the settlement.

Despite emphasis on the conference, there is an excellent brief survey of the background of the revolution, but very little attention is given to the details of the course of the revolt. The author's focus is on London, not Brussels. The work contains statistical tables of each of the concerned power's territorial mass, population, revenue, debt, and armed forces in 1830, 1831, and 1832. There is no index, but Fishman includes an excellent bibliography.

Published in 1988, the manuscript of this work was completed "some years" earlier (p. 11). The study is largely a result of a master's thesis and doctoral dissertation at Columbia University, the dissertation completed in 1972. Consequently, three major later studies (by Daniel H. Thomas, Jonathan E. Helmreich, and John E. Rooney, Jr.) are missing from the bibliography. Incidentally, none of their books cited Fishman's dissertation. Also to be noted is a lack of reference to the Archives Générales du Royaume and the papers of Van de Weyer and Charles Rogier. With these few reservations, this is a very fine work and a major contribution to the literature on conference diplomacy.

BRISON D. GOOCH  
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GEOFFREY ROBERTS. *The Unholy Alliance: Stalin's Pact with Hitler*. Bloomington: Indiana University Press. 1989. Pp. xviii, 296. \$37.50.

This book should be read through to the end, although the reader will be greatly tempted to stop at page seven and then consign the book to the "shelve and forget" category. Geoffrey Roberts claims that recently published Soviet diplomatic papers and other evidence sifted from Soviet diplomatic histories enabled him to clarify substantially our picture of Soviet-German relations from 1939 to 1941. Unfortunately, the Soviet sources Roberts mines—and he does mine everything then available—remain too scanty, fragmentary, and tendentious to ground in entirety the interpretation he lays out on pages six and seven.

Notwithstanding, the author does offer a challenging interpretation of the origins and underlying causes of the Soviet-German Non-Aggression Pact and the "Additional Supplementary Protocol" of August 23, 1939. Roberts contends that Moscow's final decision to opt for rapprochement with Germany was not made until mid-August 1939, although the Soviets had always kept in mind the possibility of some *modus vivendi* with the Third Reich. The root cause for signing the pact was the manifest failure of the collective security program; the proximate cause of the pact was the collapse of the Anglo-Franco-Soviet military negotiations in Moscow. The basis of the Soviet decision was a calculation that Britain and France might abandon the Soviet Union in the face of the imminent German invasion of Poland. Least

successful is his contention that the Stalin leadership "failed to make the most of an historic opportunity to forge an anti-fascist alliance with the West" (p. 226).

Unfortunately, *glasnost* has not yet penetrated the strongrooms holding key Soviet diplomatic papers. Consequently, the subject of Soviet-German relations remains among the most falsified, or simply blank, pages in Soviet historiography. Since 1946 Soviet authorities have denied or, more recently, questioned the authenticity of the secret protocol. Thanks to the efforts of the Baltic national independence movements and a small band of revisionist Soviet historians, however, the USSR Second Congress of Peoples' Deputies voted on December 27, 1989, to acknowledge the protocol's authenticity. While pronouncing the Non-Aggression Pact itself as legitimate, the Congress majority condemned the protocol as having violated universal moral, political, and legal norms and pronounced it null and void from the outset.

This book joins other recent publications by British scholars that reopen debate on Soviet policy and the immediate causes of World War II. Soviet scholars are now engaged among themselves and with foreign scholars in vigorous dialogue over this issue and are pressing for access to the pertinent archives. The Soviet Ministry of Foreign Affairs has announced the imminent publication of *The Year of Crisis, September 1938–September 1939*, a two-volume collection of documents, one-quarter of which were previously unpublished. At this writing we now have volume 1, covering September 29, 1938 to May 31, 1939. It does begin to fill some of the blank pages.

Alas, the copy editing on both sides of the Atlantic is unsatisfactory: "prevaricate" is printed instead of "procrastinate" (pp. 155, 194); "tortured" instead of "tortuous" (p. 222).

ALBERT RESIS  
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RALPH ROBSON. *The English Highland Clans: Tudor Responses to a Mediaeval Problem*. Edinburgh: John Donald; distributed by Humanities, Atlantic Highlands, N.J. 1989. Pp. viii, 245. \$49.95.

The history of the Anglo-Scottish borders from the fourteenth through the sixteenth centuries is a subject worth more serious scholarly attention than it normally gets. When not being ignored, the borders are usually treated as a lawless, violent frontier inhabited by colorful, if hardened, outlaws conducting cattle raids and other forays against both their cross-border enemies and their fellow countrymen. From the perspective of the metropolitan center, whether London or Edinburgh, the borders were a problem in law enforcement, ultimately solved by the growing power of the central state and, more generally, by the onward march of civilization, sweeping aside a more primitive, hence more violent, society with a presumed archaic social order based on surnames or

"clans." Occasionally, there have been attempts to see matters from the borderers' point of view, but such attempts largely accept the metropolitan characterization of border society. The only differences lie in the sentimentalizing of border life and the substitution of regret for thankfulness at its passing.

Ralph Robson's book does not get us much beyond the standard, metropolitan account and adds very little to it, even if it does try to view matters from the borderers' perspective. The subject is the English border region of Tynedale and Redesdale, in the middle march toward Scotland. Although the focus is ostensibly on the Tudor response to disorder, one-third of the book is on the medieval background. Unfortunately, Robson does not seem to be aware of recent scholarship, especially on the Scottish side of the border. In the last forty years, the study of Scottish history in general, and the medieval period in particular, has undergone a revolution. To make sense, for example, of the history of the Anglo-Scottish border, including the place of Tynedale as a property held by members of the Scottish royal house in the twelfth and thirteenth centuries, work by Scottish medievalists such as G. W. S. Barrow, Archibald Duncan, and K. J. Stringer should have been consulted. The discussion of the watershed Scottish Wars of Independence, which mark the change in Anglo-Scottish relations and hence of cross-border relations from amity to hostility, does not inspire confidence. The author's characterization of these events as a civil war between Normans shows little understanding of the conflict or of the kingdoms on either side of the border.

In general, Robson's grasp of the history of medieval border society is insecure, and he tends to use, uncritically, evidence from the sixteenth century to support assertions about earlier centuries. The treatment of the late medieval period, during which border government and society on the English side were dominated by the Percies and the Nevilles, fails to mention either bastard feudalism or good lordship. When the book finally gets to the way in which the Tudors dealt with their border problem, the reader's impression is that the main points have already been made in the work of Mervyn James and S. J. Watts.

BUCHANAN SHARP  
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THOMAS F. MAYER. *Thomas Starkey and the Commonwealth: Humanist Politics and Religion in the Reign of Henry VIII*. (Cambridge Studies in Early Modern British History.) New York: Cambridge University Press. 1989. Pp. x, 316. \$59.50.

Thomas Starkey's *Dialogue between Pole and Lupset* has long been a puzzle for historians and political commentators. The sweeping reform measures advocated in the *Dialogue* were presumably little known during



Starkey's career in the 1520s and 1530s since the work remained in manuscript. It was not published until the nineteenth century and then in an inadequate edition. Until Thomas F. Mayer's scholarly edition in the Camden Fourth Series (1989), there had been no complete text of the *Dialogue* available. Not surprisingly, commentators have varied widely in their assessment of the *Dialogue*, its place in the history of political thought, and Starkey's connections with the tangled events of Henry VIII's reign. Now, with the aid not only of Mayer's edition of the *Dialogue* but also of this detailed study of Starkey's education, career, and literary achievement, it is finally possible to come to an accurate understanding of this heretofore elusive figure.

Starkey has generally been described as an English humanist, a term that has come to seem imprecise and almost vacuous because of the indiscriminate way in which historians have used it to refer to thinkers as different as John Colet, Thomas More, and Thomas Elyot, among others. But as Mayer describes Starkey's indebtedness to Aristotle and Cicero, whose works he read at Magdalen College, Oxford, to the circle around Pietro Bembo in Padua, where he imbibed republican political ideas, to Gianfrancesco Sannazari, called della Ripa, with whom he studied civil law at Avignon, and to Jacopo Sadoleto and Gasparo Contarini, from whom he caught the spirit of *evangelismo* in Avignon and Padua, the dimensions of Starkey's distinctive humanism become clear. As Mayer shows, Starkey belonged to the Italian civic tradition as mediated through Padua and Venice, to the classical tradition in education with its emphasis on rhetoric and history, and to the Italian evangelical Catholic tradition of the early sixteenth century. Mayer argues convincingly that Starkey was "perhaps the most highly Italianate Englishman of his generation" (p. 170) and that he played a significant role in transmitting to England the ideas he encountered and made his own over the course of residing for more than a decade in Italy. Historians would be well advised to try to describe with equal precision the combination of influences that helped shape other English thinkers of the period.

As for the *Dialogue*, Mayer shows that its radical ideas—an elected monarch, whose powers are circumscribed by councils, a reformed church and educational system, a sturdy and more self-sufficient economy—are by no means egalitarian in intention, despite the author's use of a political vocabulary centering on the term "commonweal," but are part of a program for a revived nobility. Reginald Pole, who is represented in the *Dialogue* as the advocate of these ideas, was cast by Starkey as the leader of an aristocracy that would reclaim its traditional place in the constitution, with the objective of restoring a balance within the government and achieving social harmony. Written about 1529–30, the *Dialogue* was quickly overtaken by events, including Pole's defection from England to Rome and a more thoroughgoing refor-

mation of the English church than Starkey had envisioned. But the *Dialogue* is very much worth the attention of students of the sixteenth century for what it tells us about the intellectual ferment and political maneuvering in England during the turbulent 1520s and 1530s.

Mayer's treatment of Starkey's career as a writer and political adviser is lucid and richly informative. He does not spare his readers the complexities of the world of ideas and political associations that he describes. At times, the narrative and analysis seem clogged with conjecture. But reading the book is immensely rewarding.

W. BROWN PATTERSON  
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PAUL E. KOPPERMAN. *Sir Robert Heath, 1575–1649: Window on an Age*. (The Royal Historical Society Studies in History, number 56.) Wolfeboro, N.H.: Boydell, for the Royal Historical Society, London. 1989. Pp. 335. \$67.00.

Sir Robert Heath held important legal offices during the reigns of the first two Stuart monarchs. He was a loyal servant of the crown and a defender of the royal prerogative. As solicitor general, attorney general, chief justice of the Court of Common Pleas, and chief justice of the King's Bench, he was involved in some of the critical disputes of the age. As attorney general he prosecuted the Five Knights case in 1627, and two years later he was responsible for prosecuting Sir John Eliot, Denzil Holles, and Benjamin Valentine. Dismissed as chief justice of the Court of Common Pleas in 1634, he died in exile in 1649.

Paul E. Kopperman uses Heath's life and career to provide insights into the period. Although recognizing the legitimate criticism of biography as a vehicle for writing history, he defends his work as "new biography," which attempts to provide evidence for dealing with the questions that historians ask of an age through study of an individual who played a major role in some of its critical events. Heath is an ideal subject for such a study. For example, Heath's parliamentary career suggests that consensus was more common than conflict in the 1620s. The study also offers support for Nicholas Tyacke's arguments (*The Anti-Calvinists: The Rise of English Arminianism* [1987]) about the divisive effect of the Arminian faction. Even a committed crown servant such as Heath considered the views of Richard Mountague and others dangerous. He also shared in the general fear of Catholicism, which tended to unite the nation in the 1620s, but, during the reign of Charles I, he came to see radical Protestants as a greater threat.

The study also provides useful insights on the place of royal proclamations in Stuart government. Although Heath was responsible for many of the Stuart proclamations, he clearly understood their proper constitutional role as emergency legislation, and in



1629 he took legal action against the writer of a pamphlet that made it seem as if the king intended "to make and repeale Lawes and Statutes, by your Majesties Proclamations only, without consent of Parliament" (p. 210).

Kopperman has written a very useful book that provides evidence for a better understanding of the events leading to the Civil War. Heath's career illustrates how one of the loyal servants of the crown, who ended his life in exile under the condemnation of Parliament, could nevertheless sympathize with parliamentary objectives. Like so many other Englishmen of his day, Heath was fundamentally a moderate. He believed in England's ancient constitution, and he was fervently loyal to the monarchy while supporting a moderate reform program and defending the legitimate prerogatives of Parliament. Radicals such as Eliot or John Pym also accepted the traditional framework of society prior to the 1620s, but they placed a greater priority on reform and were prepared to abandon their commitment to the crown when the king failed to meet their grievances. Possibly the best indicator of Kopperman's success in writing "new biography" is the sense of tragedy one feels both for Heath's personal fate and for an age in which the strident voices of extremism overwhelmed the forces of moderation.

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RONALD HUTTON. *Charles the Second: King of England, Scotland, and Ireland*. Oxford: Clarendon Press of Oxford University Press. 1989. Pp. xii, 554. \$29.95.

On the first page of this excellent biography, Ronald Hutton candidly confesses that he is not "in love" with his subject. In resisting the winning personality that made King Charles II a "monarch fit for the company of gentlemen" (p. 446), Hutton joins a number of historians (rather than biographers) who have done likewise for over nearly three centuries. They include eighteenth-century Whig apologists, the great nineteenth-century Whig historian Lord Macaulay, and a host of twentieth-century scholars, among them K. H. D. Haley, J. P. Kenyon, and John Miller, whose views Hutton expands and sometimes contests. If Hutton is less original in this attitude than he implies, he is still the first professional historian to write a critical full-length biography of the king. And, if his refreshingly independent and sometimes iconoclastic judgments are open to debate, yet they will command respect because of his exemplary research. This study, based on "all the known surviving" sources, deserves a bibliography to enable the reader to identify the fresh sources Hutton has uncovered. The book is notably readable. Hutton provides memorable vignettes of individuals (pp. 384–85) and sprinkles his text with arresting phrases; for example, he

describes the elevation of Heneage Finch to the earldom of Nottingham in 1681 as "clearly a matter of gilding a figure-head" (p. 405).

Hutton is quite successful in fulfilling his intention of maintaining the focus on the king—no mean feat in a book that follows the time-honored "life and times" format. He also conscientiously integrates Scottish and Irish affairs into the narrative rather than relegating them to separate chapters. The result is fresh insights into English policies; for example, the decision to prorogue the first Exclusion Parliament is shown to be largely accountable to reverberations from events in Scotland (pp. 374–76). His aim of asking "questions of interest to historians" is less well realized. Addressing political, religious, and diplomatic issues at the highest reaches of kingly government is a traditional perspective that engages the author and is, of course, entirely legitimate, but it offers little or no room for questions that many historians today find interesting, such as court culture, the royal household, and the court's use of the press.

In six generous chapters, Hutton recounts Charles II's experiences from birth to the Restoration as prince or king in exile in Wales, Scotland, and on the Continent. Although avoiding psychoanalytical musings, Hutton notices the presence in youth of certain characteristics of the man (p. 7). Throughout, Hutton emphasizes the king's religious attitudes, which he concludes were self-serving and unprincipled. Charles II was neither a true Catholic, an Anglican, nor a believer in religious toleration. He severely punished Quakers, making him the "most savage persecutor of all" early modern monarchs (p. 457). Hutton's most controversial judgments concern the king's handling of the Popish Plot and the Exclusion Crisis. Arguably he overstates the personal nature of the monarchy and overstates his case when he assigns responsibility to Charles II for a "string of mistakes . . . which created" many of the problems (p. 357) that the nation faced. This proposition underrates the king's political acuity and trivializes the plot and the emergence of the Whig opposition. Hutton is also mistaken in asserting that Charles II's presence in the House of Lords "was decisive" in the defeat of the Exclusion Bill (p. 396). It was the fear of Whig radicalism and the number of peers friendly to the king, among whom were the "dead weight" of the bishops, that defeated the bill. In a final splendid chapter, Hutton offers his profile of the king, which may well persuade readers that "Slippery Sovereign" (as he calls him) describes Charles II more aptly than "Merry Monarch."

LOIS G. SCHWOERER  
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STEVE RAPPAPORT. *Worlds within Worlds: Structures of Life in Sixteenth-Century London*. (Cambridge Studies in Population, Economy, and Society in Past Time,

number 7.) New York: Cambridge University Press. 1989. Pp. xv, 449. \$54.50.

Steve Rappaport disputes some aspects of the conventional wisdom regarding life chances in the early modern city—abysmal poverty, incompetent government, and violence-ridden streets—while substantiating others such as enormous levels of mobility into and out of the city, terrifying rates of mortality, and late ages at first marriage. Rappaport's London is a place where one had a reasonable chance of success if early death was avoided. Indeed, the longer one lived, the better the likelihood that social mobility would be achieved.

Tudor London was dominated by its livery companies, which controlled the economy and regulated the labor supply. These companies also acted as civil courts and even took over some of the smaller concerns of the criminal justice system. Only after serving an apprenticeship, usually between the ages of eighteen and twenty-six, did a man present himself at the Guildhall. Ninety percent of London's men became citizens and companymen by means of apprenticeship in one of the city companies. By constructing model life tables, Rappaport compares the number of new citizens—derived from an extant register of the early 1550s—with the numbers of men being added to the population. Central to Rappaport's argument, then, is a demonstration that three-quarters of the city's adult males were freemen; this level seems to have held right down to the end of Elizabeth's reign.

There was a loosening of the medieval "custom of London" in the reign of Henry VIII, which emphasized the link between the social and economic status of companyman and the political one of citizen. Thus, the city government was able to cope with the trebling of its Tudor population. London's growth was not accompanied by massive proletarianization.

Rappaport argues that historians have been misled by the Phelps-Brown real-wage index and have not given enough credence to coping strategies, which probably meant that "Londoners who made those changes were not driven below the poverty line to the brink of starvation. After all, dining on rabbit instead of mutton is entirely different from doing without food altogether" (p. 151). Moreover, in face of moderately declining real wages—Rappaport presents his own statistics drawn from company records—it would appear that wage-earners worked longer and more often. And it would be mistaken to overemphasize the size of the proletariat: the average production unit consisted of a master craftsman working alongside a couple of apprentices and a journeyman. There was inequality, to be sure, but Rappaport's research supports John Stow's estimate that "they of the middle place . . . are first [that is, most numerous], and do exceed . . . the rest" (p. 284).

If "the greatest part of them are neither too rich nor too poor," as Stow claimed, then the way in which this "middling sort" experienced social change is a

significant problem. The most interesting part of Rappaport's excellent book concerns the career mobility of a sample population "reconstituted" from the mid-century freemen's register and cross-linked with other records. This is the major contribution of the book, and it provides a fascinating glimpse into the life chances of those who were lucky enough to survive the ever-present threat of death. In Rappaport's view, Tudor London was an open society. It would be grossly mistaken to consider it to have been a chaotic place without social services and riven with class divisions. If not every man became lord mayor, like the heroic Dick Whittington, most who survived could look forward to being householders, many could become liverymen, and a sizable minority would spend a while at the top of their company's status hierarchy. High rates of mortality kept the system of status circulating among London's citizens. "For the man who sweated out his years of apprenticeship and then journeywork, the odds were seven to one that he would become a householder and have his own shop and then were roughly one in three that one day, if he remained alive and in London, he would wear the livery and thus enter the elite of his company" (p. 387).

By probing the "worlds within worlds" that the livery companies offered to contemporaries, Rappaport suggests that many opportunities for advancement existed as did a reasonable likelihood of achieving attainable goals. Taken together with Jeremy Boulton's *Neighbourhood and Society* (1987), Vivian Brodsky's forthcoming volume on mobility and marriage, Roger Finlay's *Population and the Metropolis* (1981), and Paul Seaver's *Wallington's World* (1985), Rappaport's finely crafted study of the Tudor companyman is part of a transformation in our vision of social life in early modern London.

DAVID LEVINE

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PATRICK COLLINSON. *The Birthpangs of Protestant England: Religious and Cultural Change in the Sixteenth and Seventeenth Centuries*. (The Third Anstey Memorial Lectures in the University of Kent at Canterbury, 1986.) New York: St. Martin's. 1988. Pp. xiii, 188. \$39.95.

Patrick Collinson delivered the third series of Roger Anstey Memorial Lectures and the second John Hayes Memorial Lecture in and around the University of Kent at Canterbury in 1986. This book is the published version of these five lectures.

The author's subject is the consequences of the Reformation for an English sense of nationalism, the development of a distinctive, albeit short-lived, Protestant urban culture, the change in the character of families, and the cultural revolution in Elizabethan and early Stuart England. Collinson attacks all of these subjects with his customary verve and never

flinches from historiographical controversy. Among those he contends with are Lawrence Stone, whom he attacks for his stylized version of the history of family life in early modern England (pp. 67, 80, 83); William Haller, for his Anglocentric interpretation of the Elizabethan martyrologist John Foxe (p. 14); and recent revisionist historians of the period, whom he lambasts. The latter, he says, are like a carpenter who planes away all of the irregularities and knots from a piece of wood until left with an undifferentiated past and nothing but "a pile of shavings" (p. 84).

What Collinson gives us in these lectures is a good deal more than a "pile of shavings." His central argument is that Protestantism in general, and its radical Puritan wing in particular, significantly altered the way the Elizabethan English perceived their nation, their towns, their families, and their culture. After 1580, Collinson argues, England experienced a "cultural revolution" (p. 101) as medieval religious plays, scripture songs, and illustrations were banished during the "first ascendancy of Puritanism" (p. 98) and replaced by iconophobic and Ramist Scripturalism. Basing his conclusions on a wide reading of sermons and court and other records, self-consciously attempting to re-create "commonplace mentalities" rather than "majestic minds" (p. 123), and making generous references to the research of hundreds of contemporary scholars, Collinson presents a persuasive vision of a society that experienced a Protestant reformation that began in the 1570s and continued until it was rejected by Arminians and Laudians. Collinson has not limited himself simply to making sense of Elizabethan and Jacobean culture. His ultimate goal is to illuminate the English Civil War, which he does in his final chapter, revealingly entitled "Wars of Religion." In suggesting a link between his "cultural revolution" and the English Civil War, he has built on recent work of John Morrill, Keith Wrightson, Anthony Fletcher, and David Underdown. The effect of the Protestant revolution, Collinson writes, was fundamentally divisive and brought about the "war in our streets" (pp. 137, 146). This, he suggests, was "a necessary mental condition for the outbreak of the Civil War itself" (p. 146).

This book is a fine piece of historical scholarship that both brings together the recent work of a large number of scholars in the fields of early modern English historical and literary scholarship and presents a coherent view of one of the more murky centuries of England's history. This is not to say that Collinson always seems to have it right. He wants to find something distinctive about the Protestant view of the family but finds it difficult to detect anything more substantial than Protestantism's "intensification of certain very old-fashioned preoccupations" (p. 93). Similarly, he wants to establish a causal link between the cultural revolution inherent in the Protestant Reformation and the English Civil War but is hard-pressed to do so in other than metaphorical terms. It would make sense for the Parliamentarians and the

Royalists to have been differentiated by distinctive religious points of view, but, despite Collinson's fine book, these differences have yet to be clearly demonstrated.

MICHAEL G. FINLAYSON  
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DAVID S. KATZ. *Sabbath and Sectarianism in Seventeenth-Century England*. (Brill's Studies in Intellectual History, number 10.) New York: E. J. Brill. 1988. Pp. xiv, 224. \$60.00.

Eminent historians of early modern England such as Christopher Hill, Patrick Collinson, and Richard Greaves have given us lucid accounts of English sabbatarianism, but not until the publication of the book under review have we had a careful and thorough account of that special branch of seventeenth-century sabbatarianism known as the Saturday or Jewish sabbath. The idea of remembering the seventh day as the sabbath stems, of course, from the fourth commandment of the Mosaic Code, but a controversy developed in seventeenth-century England as to whether the sabbath was to be observed on Sunday, as the Anglicans believed, or whether it was to be observed on Saturday, as the Jews believed. But almost as important was another question when Puritans turned to the Bible for their only source of authority: how far would Mosaic legalism invade English religious life? The apotheosis of Mosaic legalism came in the 1650s with the readmission of the Jews and the parliamentary model building along the lines of the Jewish Sanhedrim.

The Saturday sabbath was largely the religious goal of a fairly small yet very active group of Seventh-Day Baptists, each of whom receives a separate chapter here. These men included the somewhat naive Thomas Tillam, who became notorious for having baptized a False Jew (actually a Benedictine friar); the versatile Dr. Peter Chamberlen (sometime court physician to Charles I and Charles II), who led three successive Seventh-Day Baptist groups in London during his long life; the staunch royalist Francis Bampfield, who founded the House of Wisdom for the study of Hebrew; Stephen Mumford and Samuel Hubbard, who established a Seventh-Day Baptist congregation in Newport, Rhode Island; and the anti-Newtonian and anti-Jewish John Hutchinson, whose followers carried his biblical literalism and emphasis on Hebrew studies to both Oxford University and King's College (later Columbia University). By the nineteenth century, however, the old Seventh-Day Baptist movement had almost disappeared.

One of the problems in a thematic book of this kind is limiting it to the specific subject, especially when it involves fascinating people such as Chamberlen and Hutchinson, who had such wide-ranging interests. On the whole David S. Katz controls his material very well, but occasionally I wish he would have general-

ized more about the Seventh-Day Baptists. For instance, did they belong theologically with the Particular (Calvinist) or General (Arminian) Baptists? Or did their political persuasions cluster anywhere along the political spectrum? There is, however, one area where Katz does allow himself to generalize, indeed to speculate, on his findings. In his epilogue he strongly suggests that the Saturday sabbath of the Hutchinsonians, aided by the millenarianism of the Millerites (followers of William Miller), ultimately triumphed in the modern Seventh-Day Adventist movement. The full development of that thesis would require another book, but meanwhile Katz has written an important study of one aspect of seventeenth-century sabbatarianism, a work that matches the fine scholarly standard of his earlier book *Philo-Semitism and the Readmission of the Jews to England, 1603–1655* (1982).

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JOHN ADDY. *Sin and Society in the Seventeenth Century*. New York: Routledge. 1989. Pp. ix, 246. \$39.95.

John Addy's book is a detailed study of the business of the various church courts in the old diocese of Chester. It is based on many years of work in the archives and an exhaustive knowledge of them. Although it contains many good stories and much interesting material, the book is largely anecdotal and lacks sustained analysis.

Addy begins with a survey of the unwieldy diocese with its various geographical and administrative peculiarities. This is helpful, although a clearer map than the seventeenth-century one provided would have been very helpful. Addy then surveys the various moral offenses that came before the church courts. He begins with the clergy and parish officers, including schoolmasters; the next section focuses on the laity and deals with drunkenness, defamation, sexual slander, fornication, adultery, and bastardy. The following section centers on matrimonial problems: child marriages, disputes over contracts, and clandestine marriages and divorces. The final section of the book examines society and the church courts.

The central problem in the book is organizational, and it is largely of the kind that ought to have been identified by a conscientious editor. One can almost see Addy going through his files choosing the most interesting cases. As a result, the links between stories and issues are often clumsy, and the progression is rarely made part of a logical argument. For instance, in the chapter "Sin and the Churchwardens," Addy moves from what happens when those chosen are unwilling to serve (pp. 48–49) to Archbishop Neile's introduction of Arminian regulations regarding church fabric (including how the churchwardens of Prescott sold gravestones and tombstones to avoid

raising a rate, pp. 49–50) to conflicts over church seats (p. 50). Nowhere does Addy explain the relationship between these subjects. Similar examples abound. In addition, Addy never offers an overview of a particular offense. It is difficult to know if the cases he discusses are typical: in the Chester diocese do more men sue for divorce from their wives or vice versa? Or are the cases where men sue more spectacular? If the former, this evidence would provide an interesting counterexample to both my own findings for Norfolk and those of Martin Ingram for Wiltshire. Each chapter ends with a brief concluding paragraph, but its separation from the evidence weakens the analysis: the book is never pulled together. Addy seems unwilling to commit himself to one interpretation of the evidence; we are told both that "the church courts were not repressive, but depended entirely upon the active co-operation of people at large" (p. 200) and that "there was opposition to church courts," which were "an irritant for the industrious" (p. 200–01). There is no clarification of these divergent assessments. Finally, he rarely refers to other recent work in the field; even Ingram's work, the most thorough recent work on the church courts, is not cited.

Addy has provided an enormous service in cataloging and sorting the papers of the Chester courts, and he has provided historians with a wealth of information. If Addy's argument fails to emerge as forcefully as it might, we are still grateful to him for his work.

SUSAN DWYER AMUSSEN  
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SUSAN STAVES. *Married Women's Separate Property in England, 1660–1833*. Cambridge: Harvard University Press. 1990. Pp. ix, 290. \$35.00.

Susan Staves has written a much-needed study of married Englishwomen's separate property in the eighteenth century. Focusing on issues of inheritance and separate estates, Staves analyzes the traditional documents of legal historians—cases, statutes, and treatises—to discover what changes occurred in the rules on women's property. Then she offers explanations for what these changes meant to wealthy women. Although the research design of this book is clear and focused, the analysis unfortunately is not. Although Staves has contributed to our understanding of eighteenth-century law and women's position in English society in a number of significant ways, her commentary is at times opaque.

Staves's most useful contribution is her discussion of the rise and fall of contract ideology in judicial opinions on wives' separate estates. Through a close reading of the law in this area, she demonstrates couples' desires to place limits on their mutual spousal obligations and the judiciary's acceptance of that desire through the enforcement of contracts between wives and husbands. Over time, however, justices



became uncomfortable with their role as policy makers for a new kind of marital relationship and moved away from accepting virtually all contracts to accepting only those that met certain conditions. The result, Staves argues, was patriarchal and traditional in its failure to grant women autonomous control over property and, more generally, their own destinies.

Less useful are Staves's discussions of rules of inheritance. Here she falls into the legal historian's trap: much time is spent explaining arcane rules on conveyancing that even Staves, a devoted student of women's legal history, found tedious. Her general thesis in this section, however, is compelling. She argues that families' reliance on jointures meant wealthy women enjoyed less financial security and more limited resources (despite their wealth) than they had in earlier times when dower was more generally observed.

In an attempt to analyze the meaning of certain legal developments for eighteenth-century women, Staves introduces a complicated system of categories. She offers liberal, neo-Marxist, sociological, and feminist interpretations of early modern rules of law and asks which categories are most useful for understanding the effect of legal change on women's lives. Staves's categories may be useful to modern legal debates, but, in applying them to the legal history of the early modern period, she risks the charge of being ahistorical. Although her goal (a feminist critical legal history of married women's separate property) is admirable, the result is the construction of a number of straw person arguments that bear little resemblance to my understanding of eighteenth-century legal history. After all, so little is known about Englishwomen's legal status before the middle of the nineteenth century that it is difficult to find accepted theories to dispute.

When Staves ventures away from legal history and into the social history of the family, entering into the lively debate over changes in aristocratic family relations, she provides a useful perspective. Eighteenth-century historians of the family would do well to heed her cautions about assuming that married women benefited from the new legal forms available to them. Although Staves's study of case law alone is insufficient to demonstrate the failure of jointures and separate estates to benefit women generally, her findings at the very least indicate that we cannot assume women gained by the mere existence of these new types of property.

MARYLYNN SALMON  
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MALCOLM ANDREWS. *The Search for the Picturesque: Landscape Aesthetics and Tourism in Britain, 1760–1800*. Stanford: Stanford University Press. 1989. Pp. xvi, 269. \$49.50

Malcolm Andrews's title refers to "picturesque" landscape in the oldest, most general sense of picturable. His "picturesque" tourists, equipped with the Claudian "picture" and tinted glasses to simulate "old master" varnish, see landscape as a series of *tranches de terre*, each within a frame, each with a stage, two side screens, and a front screen. Following on this general definition, the main part of the book, chapters 5 through 8, chronicles the "picturesque" tours of the last decades of the eighteenth century and studies their most popular sites—the Wye Valley, North Wales, the Lake District, and the Scottish Highlands. An additional chapter describes the framing and picturing devices such as the Claude Glass with which the "picturesque" tourists supplied themselves before undertaking their tours.

Only syntax is missing, and the critical commentary on the enterprise by Thomas Rowlandson's drawings. Some of Rowlandson's purely descriptive drawings are reproduced, but the issue of whether these are "picturesque" or "rococo" or how these two (in some ways antithetical) terms are related and reconciled in the work of Rowlandson as well as William Gilpin is not broached. There is some discussion of why, in sociohistorical-political, as well as "aesthetic," terms, the Wye, Lakes, and Highlands were chosen, but the political dimension brilliantly outlined by Carole Fabricant and others is not used.

The first three chapters are about the "taste" for landscape in England as it developed in the decades leading up to the tours: roughly from the late 1740s (Gilpin's tour of Stowe) to the 1780s when the famous itineraries were mapped out. Here Andrews relates the pre-Gilpin sense of "picturesque" as picturable to Gilpin's "Beautiful-Picturesque," explaining how the more precise aesthetic uses of the word derived from Joseph Addison's "novel or uncommon" (in his "Pleasures of the Imagination" papers in *The Spectator*) and picked up the name "picturesque" by way of Gilpin's waffling sense of the word. The technical term—applying to particular qualities of variety and intricacy, irritation and mixture, juxtapositions of rough and smooth—is definitively set forth by Uvedale Price and Richard Payne Knight at the end of the century.

The chapters on the theory are workmanlike and cover the ground. Aside from David Solkin's *Richard Wilson* (1982), however, and an essay from 1985 by John Dixon Hunt, Andrews makes no reference to any work on the subject since the late 1970s. (He cites Barbara Stafford's essay of 1977, not her book of 1983.) Andrews's study as a whole is a felicitous, popular survey of materials that are available in more original and speculative formulations elsewhere. The chapters on the tours are particularly informative and, not surprisingly given the accounts of the travelers, often witty. But the virtues of the book tilt in the direction of the coffee table.

RONALD PAULSON  
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PHILIPPA LEVINE. *Victorian Feminism, 1850–1900*. Paperback edition. Tallahassee: Florida State University Press. 1989. Pp. 176. \$14.95.

The thesis of this small useful book is that the aggressive activities of the well-known British suffragettes have obscured the significance of other areas of the feminist movement in the half-century before 1900. Philippa Levine's survey of Victorian feminism covers these neglected issues succinctly.

Levine has written five chapters focusing on lesser-known themes in feminist work. In all of the chapters, the influence of class in Britain is clearly recognized. Each chapter can be used as an entity in itself, although the author warns against such usage as activists and organizations tended to be interwoven.

Taking education as the first step in attaining other freedoms, Levine gives a cogent account of the difficulties of gaining any support for female education. Also defined is the dominant role of middle-class women. Clearly education had to be totally different for middle-class and working-class females. Levine sees that the new economic needs of middle-class girls for paid employment required education. The many women and organizations involved in this effort are well presented.

A short chapter on national and local politics covers the more familiar activities of the suffrage group in its fight not only for the vote but for inclusion in local politics. The class basis and the many complications of the suffrage movement are well written.

Class in British society is again seen as such a vital influence that the two chapters on work are divided into one on bourgeois campaigns and one on unions and problems of working-class women. The conditions facing all women in competition with men for work are well demonstrated. Middle-class women had to remain concerned with "respectability" when they were forced to fend for themselves, a quality that could not concern those of the working class. "To all intents and purposes, the different classes in Victorian society occupied separate worlds, and one of the major handicaps to really thorough feminist organization was recognized as having its roots in this division" (p. 105).

In the final excellent chapter on marriage and morality, Levine illustrates the feminist struggle against sexual inequality, prostitution, divorce, and violence in marriage in which women were virtually legal slaves. There is also an excellent epilogue on the areas of success and failure of the courageous women of this period. Each chapter has a helpful list of references and chronology of events. A good bibliography for further reading is included.

This is an excellent, well-researched survey of the many fronts of feminist struggle for equality from 1850 to 1900. It covers areas that have too often been overlooked by historians concentrating on the excit-

ing Pankhursts. This work should be a great asset to all courses on modern England and British feminism.

ALICE G. VINES  
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STEPHANIE JONES. *Trade and Shipping: Lord Inchcape, 1852–1932*. (Business and Society.) New York: Manchester University Press; distributed by St. Martin's. 1989. Pp. xiii, 222. \$59.95.

As a member of the 1922 Geddes committee concerned with reducing state spending, Lord Inchcape wanted to cut teachers' pay. After all, he, the self-made tycoon, had gotten by with little formal education. What worked for his generation before 1914 should, he believed, still work, and the more Britain returned to the old ways the better. But Inchcape's prescriptions for Britain's postwar ills and his management of his shipping and trading empire compounded rather than solved problems.

In *Two Centuries of Overseas Trading* (1986), Stephanie Jones proved her unrivaled knowledge of the Inchcape group. By using extensive archives and numerous government reports in this new book, she fashions an instructive portrait of its founder. The work does not systematically pursue the orthodox historiographical debate on the role of the entrepreneur in the decline of British industry or the neo-Marxist interpretation of the organization of capital and labor but addresses many of the questions that these approaches suggest.

Although James Lyle Mackay (later Lord Inchcape) was born in Scotland, his United Empire Loyalist ancestors settled in Nova Scotia as shipbuilders. An ambitious workaholic, he spent twenty years in India rising to prominence in a firm of shipping agents established by William Mackinnon, founder of the British India Steam Navigation Company (BI) and promoter of British expansion in East Africa. Despite strong racial and class prejudices he shared with other upper middle-class British entrepreneurs, Mackay discovered that cultivating Indian merchants (he learned Hindi), mixing with ships' crews and officers, and investing in Indian transport and cash crops paid off.

In 1914 Inchcape became head of the largest shipping conglomerate in the world when the Peninsular and Oriental Steamship Company (P&O) merged with BI. By then he had also served on several government bodies in India and in Britain and had even been shortlisted for the viceroy's position. He played a key role in ensuring that the state's use of private steamers and ship losses suffered during World War I did not unduly affect company balance sheets. The P&O made money during the war.

After the war the P&O lost money. But through what Jones calls "creative accounting" (p. 126), not allowing sufficiently for depreciation and diverting funds from his own personal fortune and from

subsidiary companies, Inchcape gave P&O shareholders their annual dividends. Meanwhile he strongly criticized British state activity in the economy while government-assisted foreign shippers grabbed a larger share of dwindling cargoes. He was also slow to modernize his fleets.

Jones could have probed the inner man more, his obsessions and compulsions, and used "luck" less to account for his successes before 1914, but her verdicts on Inchcape's business ethics are particularly well crafted.

ROBERT KUBICEK  
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CHRISTINE BELLAMY. *Administering Central-Local Relations, 1871-1919: The Local Government Board in its Fiscal and Cultural Context*. New York: Manchester University Press; distributed by St. Martin's. 1988. Pp. viii, 301. \$55.00.

The Local Government Board, that much-maligned stepchild of the Victorian administrative system, has at long last received the sort of serious attention it deserves. It has too often been treated as a kind of structural monstrosity or as a passive precursor to the activist, comprehensive central social agencies of the twentieth century. Placing the board in a wider context, Christine Bellamy sees it as accurately reflecting an enduring pattern of central-local relations in Britain that, in important respects, persists to the present.

The board was formed in 1871 out of a merger of the Local Government Act Office, the Poor Law Board, and the Medical Department of the Privy Council. From the outset it was riddled with divisiveness between Poor Law and medical services, between generalists and specialists, and between activists and fiscal conservatives. It also inherited a pattern of central-local relations from such earlier agencies as the Poor Law Commission. In this pattern, central government did not direct but rather negotiated with the powerful elites who dominated local government. This phenomenon, which the author refers to as "local possessive pluralism," was enhanced by the traditionalist bias of many ministers and officials, indeed by the gentry background of many inspectors.

In spite of this inertia, British government might well have generated something more closely approximating Benthamite central agencies had it not been for what Bellamy calls "the prolonged unresolved fiscal tension which accompanied the expansion of the domestic functions of the state" (p. 11). This resulted as much from the Treasury's extreme reluctance to sanction borrowing as it did from the pressure of ratepayer lobbies such as the Local Taxation Committee or the cheese-paring mentality of Poor Law boards and other local authorities. Urban corporations, frustrated in their quest for Treasury-sanctioned loans for vital improvements such as electrification and the building of tramways, turned to the

expensive but time-honored procedure of private bill legislation. This by-passing of central government tended to reduce the Local Government Board to a largely "reactive and marginal role" (p. 198). Bellamy shows that board-appointed district auditors did have some success in improving local authorities, but on the whole they were too few and too overworked to effect major "reforms" such as the elimination of outdoor relief.

Confronting such inertia and conservatism, the service delivery lobbies were able to make little headway. Groups like the British Medical Association and the National Association for the Promotion of Social Science pressed repeatedly for a ministry of health. Initially confident that the Royal Sanitary Commission of the late 1860s would recommend it, they were forced to wait nearly half a century. From their perspective, and that of most modern commentators, the Local Government Board was an obstacle to needed improvements, a two-generation barrier to progress. Bellamy shows such an assessment to be both simplistic and unfair. The Local Government Board both reflected and affirmed certain enduring elements of English constitutional practice. Moreover, some key improvements were made, in spite of the board's ramshackle nature and the tension-ridden character of central-local relations. And finally, the author asserts, modern-day practice is not as remote from this pattern as many would like to believe.

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HAROLD PERKIN. *The Rise of Professional Society: England since 1880*. New York: Routledge. 1989. Pp. xvi, 604. \$49.95.

In 1962, the historian G. Kitson Clark announced that the two words that had done most to stultify thought about Victorian England were "middle class." Harold Perkin in his earlier, justly renowned book, *The Origins of Modern English Society* (1969), took part of the sting out of that criticism by creating two middle classes for early Victorian England, one the familiar "capitalist" or "entrepreneurial" class, the other a "professional" class consisting of writers, public officials, doctors, lawyers, and the like. (Although Perkin pluralized the middle classes, he posed another problem for the historian by singularizing the "working class" in defiance of contemporary usage and of the considerable differences within that class.)

In his present book on England from 1880 to the present, Perkin brings this professional middle class to the center of the stage and makes it the star of the play. It is a provocative thesis and an extremely useful one, liberating modern English history from the stultifying weight of a single middle class, as well as from an economic interpretation of class so clearly

inadequate that even many Marxists have now abandoned it. It also liberates the historian from the heavy hand of the sociologist, for Perkin goes beyond the familiar Weberian class defined in terms of power and status by introducing the crucial factor of "ideals." The "consciousness" or "ideology" of the new class derives not only from its own class interests but also from an idea of how society should be ordered and organized—in the interest of this class, to be sure, and also in the best interest, as the class conceives it, of all of society. Thus, the collectivist bias of the class is more than a reflection of its social role; it involves a commitment to a particular social agenda, a "professional ideal."

An interesting and perhaps unintended consequence of this thesis is that it undermines the very idea of class. If this new class has become as prominent and pervasive as Perkin says, exceeding in number and importance all other classes, infiltrating all social groups and institutions (including the working class and trade unions), and "professionalizing," in effect, virtually all of English society, it loses the particular character of a class and acquires a "universal" character. (One is reminded of the Young Hegelian Marx who foresaw the transformation of the proletariat into a "universal" class representing not its particular interests but the interests of mankind, "dissolving" itself as a class while dissolving the whole of society.)

The section on the early period, 1880 to 1914, raises the most questions. Characterizing these years as "the zenith of class society," it describes a time when inequality and class segregation are said to have been greater than ever before. But Perkin presents some statistics contradicting this view. If the working-class share of the national income declined, it was because the size of the working class had shrunk as many of the children of that class moved upward into an "intermediate" class. The expansion of that intermediate class and the social mobility to which it testified suggest a diminution of inequality and segregation. (Moreover, the share of the national income that went to the upper class also declined at this time.) Perkin's view seems to support the much heralded and highly premature news of the "Strange Death of Liberal England" in the prewar years. The period as a whole, however, which witnessed the remarkable surge of social consciousness and philanthropic activities in the late nineteenth century and the momentous social legislation of the early twentieth, might more fittingly be described not as the zenith of class society but as the beginning of the mitigation of class society.

For Perkin the beginning of "class abatement" occurred in the interwar years with the rise of "corporate society"—an entente of employers, trade unions, and the state collaborating to promote the professional ideal of social harmony. The "triumph" of that ideal came after World War II when the welfare state extended "social citizenship" to the

entire population by making everyone a beneficiary and partner in the new society. Later, the welfare state saw a "bifurcation" of that ideal, with professionals in the private sector competing against professionals in the public sector, the former seeking a more privatized, less regulated economy based on equality of opportunity rather than of results. "Thatcherism" was the climax of that struggle, witnessing the first serious "backlash" against professional society. But even then professionals controlled the terms of the debate and dictated the proposals for reform.

It is with great theoretical resourcefulness and a wealth of factual detail that Perkin pursues this thesis of the professional society throughout a century of social history. One might say that no single concept can bear so heavy a burden of history, and there are moments in this book when one would be grateful for a respite. Yet the concept itself is so fruitful, nuanced, and well documented that we can only be grateful for what is unquestionably a major contribution to the history of modern England.

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EMERITUS  
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RICHARD A. SOLOWAY. *Demography and Degeneration: Eugenics and the Declining Birthrate in Twentieth-Century Britain*. Chapel Hill: University of North Carolina Press. 1990. Pp. xix, 443. \$45.00.

A detailed and richly documented history of the ideas (and of the men and women who formulated and preached them) of the British eugenics movement, this study offers an original interpretation. Developing his case from his earlier study of the late Victorian and early twentieth-century birth controllers, Richard A. Soloway sees eugenics as a cultural and social response to the British fertility decline. He follows the eugenics and population debates through to the present, illuminating the interplay of ideas and social change and, at the same time, describing the often eccentric, opinionated, and class-bound individuals who shared at least some eugenic principles.

A brief review may best illustrate Soloway's argument and document the syncretic tendencies of the movement by focusing on some of the men and women who enliven the pages of this book. All were from the middle or upper classes and well educated; throughout the long history of eugenics, heredity and the quality of human beings were the key concepts. The term "eugenics" originated with Francis Galton, a cousin of Charles Darwin, who wrote from the 1860s to 1880s a series of genealogical surveys of men of "superior talent." He applied what Soloway calls his "mathematical, classificatory mind" to the improvement of the human race, arguing that planned marriage and reproduction were needed (p. 19). His translation of Darwinism into social prescription was bolstered by statistical studies such as those of Karl Pearson (who demonstrated the decline of the Eng-

lish birth rate) and T. H. C. Stevenson (who first revealed that variations in fertility correlated inversely with class). Pearson's biometric laboratory and the Eugenics Record Office (endowed by Galton) were merged in 1906 as the Francis Galton Laboratory for the Study of National Eugenics.

In 1907, the Eugenics Education Society was founded; its early members included physicians, biological and social scientists, plus sexologist Havelock Ellis, Fabian reformer Beatrice Webb, and playwright George Bernard Shaw, but not Pearson, who preferred to separate his own "accurate statistical work" from the "high-strung, enthusiastic quacks" (his words, quoted p. 33) who flocked to the society. The society promptly entered the debate about "physical deterioration" and the "decline of the race," prompted by the discovery of the poor physical condition of British army recruits in the Boer War. Eugenics, the argument went, could reverse the trend. Proponents were challenged in their assumptions, and even supporters had doubts, as witnessed by recurring turnover in membership.

The Eugenics Education Society nevertheless built creative and sometimes fruitful alliances with individuals and groups with different ideologies. Its first encounter (before World War I) with birth control advocates in the Neo-Malthusian League, however, failed to produce a working coalition. Despite the "dysgenic" effects of the war, evidence about the improvements in lower-class health and well-being as a result of state intervention led the society to put aside its insistence on the hereditary nature of physical and intellectual inequality and support government programs for maternal and infant care. Although some members hoped after the war that the organization would campaign aggressively for "race reconstruction," the more moderate program that was adopted stressed the collection of reliable disaggregated demographic data, social reform, and birth control. Ellis, William Beveridge, A. M. Carr-Saunders, and Maria Stopes were sometime allies in this period.

In 1930, a bequest from an eccentric recluse provided a solid financial base for the then-renamed Eugenics Society. By this time, birth control had gained acceptance in the general population. The premises of eugenics activism came under vigorous attack by Socialists such as social biologist Lancelot Hogben and geneticist J. B. S. Haldane. (Haldane's colleague, Julian Huxley, however, remained a supporter.) A phenomenon of the period was the adoption of a proletarian version of eugenics by some Communists.

Drawing on its endowment, the society sponsored important scientific and applied research in the 1930s, subsidizing publication of the Galton lab's *Annals of Eugenics*, funding the Population Investigation Committee and health organizations, and sponsoring research on Volpar, a spermicidal contraceptive. By the mid-1930s, attention shifted to slowed

population growth and the specter of population decline. The popular slogan of "race suicide" prompted what Soloway labels the "scariest, worst-case scenarios in a pseudodemographic never-never land" (p. 258). The vigorous policy debate around the issue included feminists such as Eleanor Rathbone and demographer David Glass.

The principles of eugenics were thoroughly discredited by their appropriation and translation into an anti-Semitic, anti-"inferior groups" crusade by the Nazis. The Eugenics Society and its influential members declared their abhorrence for Nazi policies but did not abandon their own belief in "selective" breeding as the appropriate target for British policy. Further setbacks came with the nonselective policies legislated in the establishment of a welfare state. Although eugenicists (supported by social scientists including Richard Titmuss) made important contributions to the Royal Commission on Population (1943-48), their ideas had lost their audience both in the population at large and among scholars. Soloway closes with a brief survey of recent echoes of eugenics ideology. His engrossing and scholarly study illuminates the importance of eugenicist ideas in academic and popular understanding of demographic factors: fertility differentials by class; the dynamics of population growth and decline; and the social context and consequences of such changes—a relationship that belies any dismissal of eugenics as merely an ideology of quacks and eccentrics.

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B. J. C. MCKERCHER, *Esme Howard: A Diplomatic Biography*. New York: Cambridge University Press. 1989. Pp. xiv, 482. \$59.50.

This study of the career of one of the prominent British diplomats of this century is based on Esme Howard's papers that were discovered within the last fifteen years, a thorough examination of the official records, a careful review of the secondary literature, and a sympathetic reading of Howard's memoirs. With 1,363 notes, the documentation is exhaustive. With masterful precision, B. J. C. McKercher describes in clear prose how this English aristocrat practiced what since World War I has been known as the "old diplomacy," of which Howard's biographer emphatically approves.

Howard's father derived his wealth from railway investments, amassed a considerable estate in Cumberland, and died in 1875 when his youngest son was only eleven years old. Howard's mother, who could claim Walpole, Norfolk, and Jamaican planters in her ancestry, mixed in European aristocratic circles with her son and dominated his life until her death in 1896. Howard went to Harrow, learned seven European languages, entered the diplomatic service, and apprenticed himself in Rome, Berlin, Dublin, and



Cape Town before his unsuccessful attempts at being elected a Liberal M.P. and developing a rubber plantation in Jamaica. He had to convert to Roman Catholicism before he could marry Isabella Bandini, the daughter of an Italian prince, with whom he had a large family and much expense. Howard's connections helped him return to diplomacy in 1902. At the age of forty, Howard began the professional career that took him to Crete, Washington, D.C., Budapest, and Berne. As minister to Stockholm during World War I, he achieved considerable success dealing with Swedish neutrality and promoting British interests in northern Europe. His efforts at Versailles on behalf of Polish independence came to naught, and his ambassadorships at Madrid from 1919 to 1924 included failure as well as success. He crowned his career as ambassador to the United States from 1924 to 1930, received more decorations and titles, returned to England, and died a few weeks before the outbreak of World War II.

This biography treats in detail the major problems with which Howard was directly concerned at each of these postings, that is, meeting the right people abroad, promoting British interests, reporting to the Foreign Office, subordinating local matters to the strategic and economic goals of the British empire, and conducting diplomacy so as always to be in accord with London's policy makers whose so-called big picture he always kept in mind. Yet, given the not exactly sterling record of British policy during the interwar period, I am less enthusiastic over Howard's career than the author. McKercher so strongly approves of Howard's career, particularly the fostering of good Anglo-Canadian relations and amiable U.S.-Canadian affairs, that some smugness comes through in his enthusiastic assessment of this loyal representative of the Foreign Office and British empire. For those who remain enamored of that legacy, however, McKercher's scholarly biography will provide much satisfaction.

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ALEC CAIRNCROSS and NITA WATTS. *The Economic Section, 1939–1961: A Study in Economic Advising*. New York: Routledge. 1989. Pp. xii, 372. \$56.00.

Few modern historians have had the privilege of witnessing the birth of economic policy, from the inside. In this detailed volume, replete with personalities and character sketches and swamped with economic facts from committee minutes, two former participants inside the Economic Section—the British government band of professional economists who exerted influence over official economic policy after 1939—describe what it was like to be near the center of decision making.

Sir Alec Cairncross, one of Britain's leading economists, and Nita Watts are the authors of this book,

which ranges over British policy decisions from 1939 to 1961 (when Cairncross himself took over as director of the Economic Section). The Public Record Office has released, under the thirty-year rule, the minutes and memoranda of the Economic Section, and, together with their own memories of events as they took place, Cairncross and Watts build up a picture of the historical scene from the early days of World War II to the budget of 1959.

The economic staff of the Stamp Survey (of war contingency plans) of 1939 was divided in 1940 into two staffs, serving the Central Statistical Office (a vital organ of research) and the Economic Section, both of them created at the same time, both of them in the War Cabinet Offices. (In 1953 the Economic Section merged with the Treasury.)

The Economic Section was involved in most of the government's policy decisions, external and internal, from the institution of "points" rationing for foodstuffs in Britain (the work of Lionel Robbins), to the economic conference in May 1943 at Hot Springs, Virginia, to the Bretton Woods conference in New Hampshire in 1944, to the White Paper on Employment Policy that led to the Beveridge Plan in 1944, to Britain's sorry economic disaster of 1947, when Sir Stafford Cripps, in the (newly created) post of minister of economic affairs and chancellor of the exchequer, managed to restore public morale and direct a policy of "austerity" with strict economic controls and continued rationing. He was successful in controlling inflation and keeping up social welfare and full employment. But in 1949 Cripps had to devalue the pound. The director of the Economic Section in those days was Robert Hall, a key figure in this book and an admirer of Cripps.

The devaluation of 1949 was succeeded by tumultuous events for two or three years, piling one on top of another, such as the Schuman Plan for a coal and steel community in 1950, the Korean War and rearmament in 1950–51, and the balance of payments crisis of 1952. In that exchange crisis, the Bank of England encouraged the "Robot" scheme: the idea of "freeing the pound," that is, letting the pound float, blocking sterling balances, and introducing convertibility for nonresidents of the sterling area. Hall and the Economic Section were opposed to Robot. The plan was subsequently set aside by the Cabinet.

The Economic Section under the fourteen-year leadership of Hall (1947–61) was plainly Keynesian all the way. John Maynard Keynes was himself in the Treasury at one time, and his influence over policy, through his writings and his disciples, was enormous, even after his death in 1946. The Economic Section took little part in demand management through monetary policy, but in demand management through fiscal or budget policy, it played a great role. Chapters 15 and 16 are important for historians seeking a clue about budgets from 1947 to 1959.

The highlights of this book are the brief but nonetheless informative sketches of leading personalities



and economists; these glimpses, and the way the authors can smile at themselves while writing them, make the book enjoyable. The personalities are all here, in a partial "who was who" of British economic life from about 1939: among them, Keynes, Hubert Henderson, Harry Campion (director of the Central Statistical Office, 1941–67), William Beveridge and Norman Chester, Austin Robinson, and the first four directors of the Economic Section—John Jewkes, Lionel Robbins, James Meade, and Hall. Their interaction with each other and the academic community and their relationships to Cabinet ministers (who alone decide policy) make up the gist of modern economic policy making.

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BRIAN LORING VILLA. *Unauthorized Action: Mountbatten and the Dieppe Raid*. New York: Oxford University Press. 1989. Pp. xiii, 314. \$27.50.

On August 19, 1942, some six thousand troops, mainly Canadian, attempted to seize the French Channel port of Dieppe. This "largest raid in history" (p. 7) was easily repulsed, and two thousand seven hundred of the would-be invaders were killed or captured.

Brian Loring Villa, a historian at the University of Ottawa who received his training at Harvard under William Langer and Ernest R. May, has devoted almost a decade's research not to the raid itself but to why it was undertaken. The book reads like a first-rate detective story and takes one into the center of British war policy in 1942.

Villa's central conclusion is that the Dieppe raid took place, after its initial cancellation, as the result of a personal decision by Vice Admiral Lord Louis Mountbatten, the youthful, newly promoted and appointed chief of combined operations. This initiative was not approved but was tolerated by higher authority, notably the Chiefs of Staff Committee. How could this have happened?

For the chiefs of staff, Dieppe seems to have been peripheral but to some degree useful as a lesser evil. "The size of the operation was determined not by weighing the military objectives but by estimating what size operation might register . . . as a decent gesture, while still not jeopardizing the integrity of [Army General] Brooke's plans for waging war that summer" (p. 87). In similar fashion, Admiral Sir Dudley Pound, whose position was precarious, found a raid on Dieppe at any rate preferable to the other alternatives being pressed on him, notably renewed convoys to Russia or a campaign in Norway, particularly if the operation did not involve the commitment of a capital ship. For Chief of the Air Staff Air Marshal Sir Charles Portal, "wasting the Luftwaffe" (p. 158) through commitment of primarily fighter

aircraft, thus preserving the bomber offensive, was the primary objective.

At another level, strategic policy, interallied relations, public opinion, and domestic politics intersected and reinforced each other. The raiding operations concept, assistance to the Soviet Union, and the "Second Front Now" movement, supported by Lord Beaverbrook as well as by much of the British Left, combined to make something like Dieppe, when pushed by determined and ambitious commanders, difficult to resist. The outcome was the wastage of a Canadian division and many recriminations (Villa devotes a chapter to the way in which the Canadian command in Britain, forbidden to go to the Middle East by Canadian policy, maneuvered itself into being chosen for the assault). The soaring career of Mountbatten, charmed and royal, was apparently unaffected.

This is one of the few books on World War II that really demonstrates the interplay between politics, policies, personalities, and military operations. Although not particularly edifying, nor its thesis demonstrable beyond reasonable doubt, the volume remains instructive on the effects of politics, domestic and military, on the conduct of war.

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DAVID HOWELL. *The Politics of the NUM: A Lancashire View*. New York: Manchester University Press; distributed by St. Martin's. 1989. Pp. vii, 231. Cloth \$49.95, paper \$19.95.

In this volume the prolific David Howell examines, from the point of view of a militant miner from Lancashire, the politics of the National Union of Miners (NUM) with particular reference to the great strike of 1984. He adopts this regional perspective because, it gradually emerges, the Lancashire area and the miners' lodges within it were a microcosm of the NUM and British coal-bearing regions as a whole.

Howell carefully establishes and assesses the situation within the Lancashire branches and the NUM more generally during the years leading up to the strike. Like the union as a whole, the Lancashire branches suffered numerous pit closures during the 1960s but devised no strategy to oppose them. In 1971 the area provided the union with its national president, Joe Gormley. He was a man of the political Right, at least in NUM terms, which meant that he offered unswerving support to the Labour party. Yet he broke decisively with the dilly-dallying that had characterized the union during the previous decade. Under his aegis the union won three great victories: two strikes for higher wages in 1972 and 1974 and the withdrawal of the Coal Boards' proposals for pit closures in 1981. These triumphs were all at the expense of Conservative governments. Yet they did

not strengthen the wing of the union with which Gormley was associated. When he retired in 1981, a man of the far Left, Arthur Scargill, replaced him.

Despite his successes, Gormley had not discovered a strategy for dealing with pit closures demanded by a government determined to implement them. During the 1970s, Edward Heath's governments had lacked grit; Margaret Thatcher's in 1981 was not yet ready for a showdown. Three years later, with Gormley gone and Scargill in his place, Thatcher's government was ready, with tragic results for the NUM. Most of Howell's book is a grim, unsentimental analysis of the great struggle that preceded the miners' devastating defeat.

Howell shows, first and foremost, that the NUM was no monolith during 1984 or anytime else. As he puts it, "The union was under attack from without and divided within" (p. 216). It fractured along political and regional and even generational fault lines. Young miners in the threatened pits were more militant than their elders, some of whom welcomed the government's "golden handshake." Lancastrians, to give just one example of regional conflict, distrusted Yorkshiremen—and vice versa. Most divisive was the question of whether to hold a national ballot to determine if the union should call a strike at all. This issue, which received the most publicity, nearly tore the union apart.

All the more remarkable then that the NUM displayed an amazing solidarity once the pickets were up. Miners may not have wanted to strike, and if given the chance would almost certainly have balloted against it, but the vast majority simply would not cross picket lines. It was this stubborn adherence to an ethos of mutuality that, despite everything the government could do to undermine it, kept the strike going for nearly a year.

This is a book for people who already know something about British trade unionism and politics. Howell effectively locates the strike within the history of the NUM, but he is less careful to situate it within the current British political scene, possibly because he thinks its place self-evident. The broad aims of Scargill, Thatcher, and Ian MacGregor are given short shrift. One suspects, however, that this book will be read with care in the beleaguered coal fields of Lancashire.

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JANET A. NOLAN. *Ourselves Alone: Women's Emigration from Ireland, 1885–1920*. Lexington: University Press of Kentucky. 1989. Pp. xii, 133. \$16.00.

This book is a succinct (ninety-six pages exclusive of notes) but well-developed argument about the origins of Irish women's immigration to the United States in the late nineteenth and early twentieth centuries and its relationship to changes in women's status. At a

time when immigration to the United States was heavily male, more than half of all Irish immigrants were women, most of them young and single. Janet A. Nolan argues that these women did not immigrate as dutiful wives and daughters transplanting Old World family structures. Nor did they come as feminists rebelling against those structures. They came as "restorationists" (my word, not Nolan's), hoping to recapture the social status and economic roles that they had enjoyed in rural Ireland before the Great Famine and subsequently lost.

According to Nolan, women in prefamine Ireland had favorable positions in the family and society. They chose their own husbands, usually marrying for love, and enjoyed considerable social freedom. After marriage their status in the family was high because of their economic importance. While husbands farmed to provide food for the family, wives kept livestock or engaged in handicrafts or agricultural labor to provide the cash that paid the landlord. After the Great Famine of the 1840s and 1850s, however, women's status deteriorated. Because impoverished men now married late or not at all, women were left with no prospect of having their own homes and the autonomy and adult status that came with them. In addition, a new social conservatism (partly inspired by the Catholic church) stifled women's social freedom, and changes in the rural economy made their traditional means of earning cash obsolete.

Nolan goes on to argue that, by the end of the century, both sexes experienced a revolution of rising expectations caused by better education, communication, and transportation and an economic revival. Women had less opportunity than men, however, to fulfill those rising expectations in the homeland. By immigrating to the United States, Nolan argues, women could. With the money they earned as live-in domestics in the United States, they could help their families of origin, pay for the immigration of relatives, and provide themselves with dowries. They could enjoy greater social freedom than in the homeland and could once again marry when and whom they chose.

Nolan emphasizes, as many historians do not, the impact of women's emigration on the homeland. She argues, for example, that women's remittances helped perpetuate the obsolete pattern of rural Irish life that prompted women's emigration in the first place. She notes, too, that heavy emigration of women of child-bearing age deprived Ireland of the laborers and consumers needed for urban, industrial development.

Not everyone will agree with Nolan's interpretations. I am less certain than she is, for example, that women's status within the rural family is directly proportionate to the extent of their contributions to the household economy, in Ireland or elsewhere. More important, Hasia Diner's book *Erin's Daughters* (1983), which covers Irish immigrants in the middle decades of the nineteenth century, makes a convinc-

ing case that Irish women's choices regarding employment and marriage in the United States were a continuation of patterns established in Ireland during the famine years rather than a departure from them, as Nolan suggests. Nevertheless, this study provides a stimulating thesis and a wealth of information about the pre-immigration background of an important group of women. Similar studies are needed on immigrant women of other ethnic groups.

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ELIZABETH ARMSTRONG. *Before Copyright: The French Book-Privilege System, 1498–1526*. (Cambridge Studies in Publishing and Printing History.) New York: Cambridge University Press. 1990. Pp. xvii, 317. \$59.50.

The regulation of the book trade has long generated interest. Elizabeth Armstrong seeks to document initial governmental interventions by examining the privilege system that emerged. This first involvement did not, as might be expected, evolve from the desire to restrict publication; rather, authors and publishers sought privileges to keep others from publishing the same manuscript, thus diminishing the possibilities for profit. Although applicants for privileges might have had to have the contents of their works approved, this evaluation was merely part of the process rather than a fully developed system of control.

The bulk of this book meticulously analyzes the 463 privileges issued from 1498 to 1526. Since only one was granted before 1507, the book actually focuses on a twenty-year period. Armstrong carefully examined the books published during the entire interval, locating as many printed privileges as possible. These recorded privileges provide the main, but not the only, source that the author subjects to a multitude of questions. One chapter, indicating the competing jurisdictions in France, describes the efforts of the king as well as those of various royal judicial officials. Those wishing a privilege apparently selected one of these authorities, conscious of the special needs of their publication, for protection. Interestingly, this haphazard system seems to have worked well with few privileges violated. In those odd cases, the confiscation of offending volumes constituted the penalty.

Armstrong also describes the justifications applicants gave in their requests for a privilege. Although the appeals were primarily financial, they used several different reasons. Publishers stressed the expense of acquiring a manuscript, whereas authors averred their need for a just recompense. Apparently, however, before granting a privilege, the authorities took most care to ascertain that the book in question was either original, out of print, newly translated, or greatly revised. Once granted, privileges could, as in the case of a newsletter, last only a few days. Some ran for ten years, but two or three

years was most common. In addition, Armstrong discusses, among many other matters, the procedures of various parts of the government in approving privileges, the location within the book of the printed privilege itself, and the reckoning of the dates for the grant of exclusivity.

The author also considers the subjects of the books published. She points up the shared dominance of religious and legal texts followed in importance by history and current events, philosophy, literature, and, in a single category, education and commentaries on classical texts. To explain why some genres outstripped others in number of books published, Armstrong emphasizes various anomalies that led authors and publishers to seek privileges.

The care and rigor that went into this book can scarcely be matched, but the author lost an opportunity to use this phenomenon as a window onto broader issues. The conclusion that the subjects represented in privileged books appear as a result not of demand but of happenstance makes it impossible to use this source to explain readers' interests. Moreover, Armstrong reaches the generally negative conclusion that these privileges bore little in common with the modern concept of literary property, although they did represent something of a step in that direction. But merely the way the authorities seemingly marched in concert is revealing about early modern France. And the justifications used by supplicants hold potential for bold analyses. Surely, privileges were a phenomenon pregnant with possibilities that the author should have exploited.

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ROBIN BRIGGS. *Communities of Belief: Cultural and Social Tension in Early Modern France*. New York: Clarendon Press of Oxford University Press. 1989. Pp. vii, 423. \$79.00.

The ten essays collected in this volume deal with the social context of religious beliefs in the "long seventeenth century of the first three Bourbon kings of France" (p. 381). Topics covered range from witchcraft and popular revolt to the institutionalization of Catholic reforms. Each of Robin Briggs's essays represents an admirable synthesis of original research and historiographical commentary, and his book provides an excellent survey of the current state of various questions concerning the history of French religion and society in the *ancien régime*.

Three chapters on witch beliefs effectively convey the "village atmospherics" that surrounded witchcraft accusations, especially the close relationship between diagnoses of harm caused by witchcraft and remedial beliefs in the possibility of magical healing, paradoxically sought from the suspected witch herself. The difficult question of why anyone ever made an untortured confession to the crime of witchcraft is illumi-

nated by analysis of trial records from Lorraine containing just such "spontaneous" confessions. Briggs documents the complex economic and psychological roots of confessions to diabolical pacts and Sabbath meetings, presenting an original and convincing portrait of the process by which poor, solitary women might come to accept the projected label of "witch" and see themselves as others saw them.

The normality of witch beliefs and accusations at the village level contrasts strongly with entrenched skepticism among the mainstream French judiciary. Using Alfred Soman's compelling data on the high rate of judicial reversal in appeals of witchcraft cases before the *Parlement* of Paris, Briggs underlines the judicial differences first pointed out by Robert Mandrou between the lower, local, and peripheral courts and the central appeals court. What needs to be explained, he argues, is not the fact of the witch panic itself (which seems inevitable given the convergences of elite and popular fears) but rather the relatively low level of witchcraft prosecution in most of Europe and especially in central France.

This concentration on the local level of events also characterizes a long essay on "popular revolt in its social context," which focuses on tensions between "vertical and horizontal solidarities" in insurrections from the 1590s through the Fronde (helpfully mapped on p. 110). Beginning with a review of the debate between Boris Porchnev and Roland Mousnier, Briggs emphasizes the fragmentation of political interests in a "complex and divided society, placed under intense pressure by the demands of royal fiscalty" (p. 174) as local elites diverted tax demands toward their subordinates.

The real core of Briggs's book lies in part 2, "Agencies of Control," which contains six essays examining "religion, repression and popular culture" (p. 381) in the French Counter Reformation. Chapters include "Church and state from Henry IV to Louis XIV" and "The catholic puritans: Jansenists and rigorists," examining Jesuit and Gallican response to reformist accusations of moral laxity. Briggs has made a major contribution to the social history of religion with essays on "the church and family" (full of amazing quotations on the church's view of marriage) and "the sins of the people: auricular confession and the imposition of social norms," both of which stress the gap between popular cultural norms and the rigorous morality preached by seventeenth-century reformers. He accepts Jean Delumeau's gloomy depiction of the religious mood of early modern Catholicism (in *Sin and Fear: The Emergence of a Western Guilt Culture, 13th to 18th Century* [1989]) but rejects the idea that the church actually succeeded in imposing its somber requirements on the population.

In his conclusion, Briggs presents a Freudian gloss on the seventeenth century as "the great repression" in which "superego controls" were replaced by more modern "internalized self-control" (pp. 408–11). This

section draws heavily on Norbert Elias and seems somewhat out of place after the carefully circumscribed generalizations of the rest of the book, but the author manages not to be too heavy handed with his psychoanalytic insights. The volume demonstrates Briggs's formidable range of expertise on a series of compelling and current historical issues and provides a crucial point of future reference for scholars of early modern religion and society.

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GUY LEMARCHAND. *La Fin du féodalisme dans le pays de Caux: Conjoncture économique et démographique et structure sociale dans une région de grande culture, de la crise du XVII<sup>e</sup> siècle à la stabilisation de la Révolution (1640–1795)*. Foreword by MICHEL VOVELLE. (Commission d'histoire de la Révolution française mémoires et documents, number 45.) Paris: Comité des Travaux Historiques et Scientifiques. 1989. Pp. x, 661. 320 fr.

Was the French revolution the social and economic transition from feudalism to capitalism? Those familiar with Guy Lemarchand's earlier work will not be surprised that his synthesis of a lifetime of research and writing on early modern upper Normandy argues that it was. This dense study has many fine points, notably its broad statistical base and a superb description of the eighteenth-century economic growth in the *pays* of Caux. Unfortunately, the book seems to have been rushed into print in order to lend polemical (and statistical) weight to the debates of the year of the bicentennial. The sense of hurriedness is evident everywhere, from the analytical unevenness (especially on the clergy) and the failure to use the relevant recent historiography to the many typographical errors, especially in the tables.

Lemarchand's extensive research in demographic and financial records shows the low level of peasant landownership that facilitated the dramatic shift from an essentially agricultural economy to a mixed proto-industrial one. In 1650, the poor of the *pays* of Caux were day-laboring peasants; by 1750, they were cotton weavers (far better off than their day-laboring ancestors). Lemarchand's broad-based presentation of the booming multisectoral economy of the eighteenth century is the highlight of the book. The chapter "Les Moyens de sauvegarde du féodalisme" is also outstanding, particularly on the nature of divisions within the peasant community.

The economic and social analysis rests on a broad spectrum of research: tithe and lease records, *terriers* and *aveux*, taille, capitation, and *vingtième* rolls. Lemarchand's presentation of post-1660 tax levels is sound, but his comparison to the early seventeenth century is undermined by his misreading of a key document of 1612 and by his failure to use the many other documents of the period from 1612 to 1660 (which he erroneously states do not exist). Direct



taxes merely doubled (unadjusted for currency devaluations) between 1612 and 1660; they did not quadruple (p. 153). Norman tax assessments peaked in the late 1630s and early 1640s (per hearth assessments at the Cauchois parish of St. Ouen-de-Breuil reached forty-seven pounds in 1645 as against twenty-nine pounds in 1665). The levels of the 1660s represented a substantial reduction, so Lemarchand's use of the 1660s as a base for his comments on the evolution of taxation fundamentally warps his long-term perspective.

Lemarchand takes great pains to emphasize the revolution as the social and economic transition from feudalism to capitalism, not merely as a political event. The very organization of the book mitigates against this central argument. The sections on the periods 1650–1718 and 1719–89 focus on economic, demographic, and social history; the revolution section summarizes these factors and offers a chronology of local events during the revolution with special emphasis on popular discontent. In Lemarchand's presentation, the revolution is essentially a political event; his remarks about its social and economic impact are formulaic and unconvincing.

Lemarchand's evidence does much to support Pierre Goubert's contention that by 1750 the *ancien régime* was dead in a social and economic sense. Lemarchand makes a convincing case that the *pays* of Caux underwent a fundamental social and economic transformation between 1650 and 1750. The older socioeconomic structure, which he calls feudalism, was dead by 1750, but its legal and political superstructure lingered on. In Lemarchand's description, although not in his analysis, the revolution was the practical and ideological destruction of that legal and political superstructure and its replacement by a superstructure more amenable to the nascent capitalist structures his documentation (although not his analysis) shows was already in place.

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HENRY VYVERBERG. *Human Nature, Cultural Diversity, and the French Enlightenment*. New York: Oxford University Press. Pp. xii, 223. \$34.50.

Thirty years after publication of his well-received book *Historical Pessimism in the French Enlightenment*, Henry Vyverberg has published another slim volume, a work that is clearly the distillation of the thought of a man who has spent the intervening thirty years teaching the Enlightenment and pondering it. At first glance the present book seems a curiously hesitant one, full of transitional conclusions.

The scholarship, however, that informs the book is firm. Vyverberg states at the outset that he will examine the period from 1748 to 1778, which lies between the publication of Montesquieu's *Spirit of the*

*Laws* and the death of Voltaire. Thirty years—the span of a generation according to the philosophes. Can a single generation really represent an entire century, an entire age? The Enlightenment still remains one of the most debated, controversial historical periods, whether for France or for all of Europe. Isolating a tiny slice of it and placing it under a powerful microscope may still not reveal the body from which it was taken. The author makes a very persuasive case for his choice, reminding us that these thirty years are also the thirty years during which the *Encyclopédie* was begun and completed by a “society of men of letters who were among the most representative minds of the French Enlightenment.” If one accepts that the *Encyclopédie* “represented the Enlightenment body and soul,” then Vyverberg's choice is surely justified (Robert Darnton, *The Business of the Enlightenment: A Publishing History of the Encyclopédie* [1979]: 522). Vyverberg's main concern is the ambiguity that he perceives in the Enlightenment: did the idea of human nature as one make it impossible for the thinkers to appreciate cultural and historical diversity (p. 20)? Vyverberg concludes, albeit hesitantly, that Encyclopedists and philosophes did recognize “natural” differences. This is perhaps best expressed in the concept of national character. “The existence of national character was virtually a truism of the Enlightenment” (p. 81). Vyverberg argues, after a close examination of the role of voyages, climate, and institutions. The chapters on “cultural miscellany,” focusing on Russia, the Jews, blacks, and Indians, as well as the chapter “The Mildly Exotic East” serve as examples of the philosophes' understanding of national characteristics.

For his concluding chapters, Vyverberg turns to attitudes toward history. He raises the tantalizing question of how historical-minded the philosophes were. Did they lack a historical sense or did they open up new ways of dealing with history? He feels that it is indeed possible to demonstrate that the French Enlightenment often showed a radical lack of historical sense. But it seems to me equally safe to say that they had a very real understanding of historical method, subject matter, and research techniques and that these are clearly demonstrated not only in Voltaire's works but throughout the *Encyclopédie* as well. Their contribution should be judged in relation to the centuries that preceded them. Instead of faulting them for stopping far short “of what we usually consider our still more enlightened appreciation for diverse civilizations” (p. 204), we should acknowledge that they broke many new paths. Vyverberg insists on their “relative sociological and historical ignorance” (p. 206). His conclusion grants the Enlightenment “occasional victories” (p. 206). Yet the battles won were hard battles, and the victories were real.

The book is not easy to read, but it is rewarding. To anyone who wishes to become acquainted with the



multiplicity of Enlightenment thinking, it provides an erudite introduction.

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ALAN FORREST. *The Soldiers of the French Revolution*. (Bicentennial Reflections on the French Revolution.) Durham: Duke University Press. 1990. Pp. xx, 224. Cloth \$32.50, paper \$8.95.

Alan Forrest's book on the military history of the French revolution is the first in the series edited by Keith M. Baker and Steven L. Kaplan, whose intention is to present a dozen volumes providing a synthesis and reassessment of major aspects of the French revolution. The editors are seeking to build a new understanding of the revolution in place of the "Marxianized republican interpretation" (p. xvi) of Georges Lefebvre and Albert Soboul. Because attacks on the established orthodoxy have been much more successful than attempts to create a new interpretive framework in recent years, one can wish Baker and Kaplan well—while maintaining a skeptical attitude toward their chances of success.

Forrest's book sets the series on a promising course. It unobtrusively displays a mastery of old and new secondary material—including the works of "Marxianized" republicans—and a familiarity with national and local archival resources (that have so often been the primary casualties in the historiographical conflicts). After an introductory chapter, the author discusses six areas: the government's military policy, the reorganization of the army, recruitment and responses to recruitment policies or practices, political propaganda and military morale, providing and caring for the troops, army life, and civil-military relations. Throughout, his primary emphasis is on the impact of policies and problems on the soldiers and officers and their responses to them. What emerges is a human history devoid of esoteric musings that divert the readers' attention to the historian rather than the topic. The result is a clear, concise synthesis that conveys a genuine understanding of the subject, the soldiers of the French revolution.

This is not to say that Forrest is unaware of conflicting interpretations or that he does not have any of his own. On the contrary, he examines the premises and conclusions of previous works, evaluates them, offers the results of his own research—especially in departmental archives—and presents his judicious analyses. The consequences should not be surprising to practicing historians. Forrest revises and refines the conclusions of earlier researchers without ignoring their contributions and provides a more nuanced—and probably more accurate—interpretation of the issues. For example, Forrest is especially informative about popular responses to recruitment policies between 1792 and 1794. He points out the exaggerations and misconceptions about the patriotic

enthusiasm of the French people during this crisis. At the same time, he appreciates that the surviving testimony of some patriots cannot be ignored; revolutionary dedication did exist and play a role, even though it was not as pervasive as some have claimed.

In short, this book is the kind of work that most historians should aspire to write. It is not a pioneering study, although it certainly adds to historical comprehension, nor is it a major reinterpretation, although it clearly changes ideas about its subject. It is fundamentally a useful contribution to better understanding an essential aspect of a critical historical phenomenon. The book and the series in which it appears are also evidence that the end of the French revolution, like the report of Mark Twain's death, is an exaggeration.

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LINDA ORR. *Headless History: Nineteenth-Century French Historiography of the Revolution*. Ithaca: Cornell University Press. 1990. Pp. xiii, 185. \$28.95.

Literary critics have colonized a neglected province of the historians' empire: the classical texts inherited from previous practitioners. "Historians had all but abandoned the monstrous, monumental histories of the nineteenth century," writes Linda Orr, "implying that they were useless, if not silly" (p. 117).

The author of *Jules Michelet: Nature, History, and Language* (1976) continues her analysis of nineteenth-century historical texts, wielding the same impressive and sophisticated weapons of literary criticism but with a difference not just of breadth but of tone, style, and, yes, discourse. From title to conclusion this is a personal book that turns on several conceits. Orr twice explains her title. Early on (p. 6, n. 5) she writes that she will not analyze Anne-Louise de Stäel's *Considerations sur la Révolution française*, "thus cutting off my own origins, or the head of my book." Later (pp. 75–76) she argues that Jules Michelet fades into the narrator of his *Histoire de la Révolution française* and in turn "fuses with the characters in the plot." Thus, "history or the real . . . is what speaks all by itself, alone. The events seem to write and read themselves: a headless history."

Michelet became a character in his book. So does Orr. She tells us about her physical and emotional reactions to reading the Romantic historians. Her "head started to spin" when she "sat down and tried to make sense of Alexis Tocqueville's incredible sentences" (p. 91). She was "first drawn to the image of the Reporter's-Box . . . partly from a knee-jerk impulse to examine everything that focused on writing per se" (p. 117), and, she writes, "I vowed I would not end up with a definition of history, and now look what I've done" (p. 159).

Amid these conceits (and the jargon of her discipline) there are brilliant moments. Orr traces

Michelet's mutilation of Maximilien Robespierre's last speech back to the Jacobin gutting of Georges-Jacques Danton's defense (pp. 83–85). Her discussion in chapter 4 of Tocqueville's *L'Ancien régime et la Révolution française* is, I think, the best part of the book. Analyzing Tocqueville purely as a writer, Orr abandons more traditional approaches and says some provocative things about this sanctified text. She insists that Tocqueville's language is "possessed by a physiology as gruesome as Michelet's" (pp. 108–09) and that his "metaphors, parodies, long sentences, and aphorisms do not add up to a theory of history" (p. 116), and his work is no less ambiguous than the largely ignored histories of his contemporaries.

Orr's book raises fundamental questions about the craft and nature of history. As historians have become more analytical, literary critics have dissected our narrative heritage. This change has not brought the disciplines closer. For Orr, history is essentially a form of discourse; it has no life independent of itself and its language. The discourse is not about an external past: Orr does not consider the past innately interesting. She is concerned with texts and assumes that to understand the text is to understand the past. Whether her assumption is true or not, it is not the historian's assumption.

The irony is that historians also work with texts, but for different ends. Their discourse rests on the evidence and is determined by the raw materials. Orr seldom looks at what lies behind Tocqueville's or Michelet's histories. Her texts are racked for literary meaning, not for what they tell us about the French revolution in the nineteenth century. In the process the French revolution dissolves into image and metaphor, rhetoric and language, and a subject of historical writing rather than a complex episode both primal and transcendent.

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ROBERT JUSTIN GOLDSTEIN. *Censorship of Political Caricature in Nineteenth-Century France*. Kent, Ohio: Kent State University Press. 1989. Pp. xii, 293. Cloth \$28.00, paper \$18.50.

Robert Justin Goldstein salutes the role of visual images in undercutting the legitimacy of the regimes in nineteenth-century France, part of the dialectic of revolution and reaction. "Caricatures," Goldstein argues, "were viewed as even more dangerous than the print press partly because they were perceived as simply being a more powerful means of communication" directed at a mass—and in many regions still largely illiterate—audience. In the words of a minister of justice who presented a strict law of censorship, caricatures speak "to [the] eyes" of the people (p. 2).

The reign of Louis-Philippe has never, to this day, recovered from memorable visual attacks launched

by its enemies to the Left: "Before overthrowing a regime, one undermines it by sarcasm, one casts scorn upon it," asserted a magistrate prosecuting *Le Charivari* in April 1835 (p. 6). The first years of the July Monarchy were the heyday of Honoré Daumier and Charles Philipon of *La Caricature* and *Le Charivari*. This "war of disrespect" was highlighted by the damning sketches of the portly monarch sketched as a pear, such that all images of pears and images by virtue of similarity, such as rabbits viewed from behind, were subject to censorship. But the authorities could not do much about the pears hastily sketched on the walls of Paris. The year 1835, which saw one of the more spectacular attempts to assassinate Louis-Philippe, also brought a new censorship law. An official declared that "there is no more direct provocation to [commit] crimes" (p. 6) than caricatures. This view remained basically that of the henchmen of subsequent regimes into the Third Republic. This is Goldstein's story, and it is nicely told.

Much of this is known, but Goldstein is justifiably proud of presenting the "first comprehensive study of censorship of caricature in nineteenth-century France" (p. 280). Indeed, his book provides useful information on the laws and mechanics of censorship and of the trials and tribulations—including imprisonment in Saint-Pélagie and less pleasant places as well—of caricaturists who ran afoul of censors and prosecutors. The author is quite good on the strategies and subterfuges of publishers and caricaturists and of sustaining symbols of resistance, including the image of the *éteignoir* or candle snuffer, extinguishing light on behalf of the authorities, and of the censor herself, Madame Anastasie, a female image, like that of the republic itself.

Goldstein includes some splendid vignettes. For example, as artists were placed in the awkward situation of having to obtain the permission of those they wanted to caricature, not all public figures reacted as positively as the person who wrote, "in case you take a fancy to portraying me in your journal, you can do with my head what you want, I deliver it to you" (p. 20). There was Marshal MacMahon, who refused to allow himself to be caricatured, not caring for such depictions as that in 1877 showing him on his horse with the caption "That horse has an intelligent look, indeed" (p. 205). The author relates the sad story of Sebastien Peytel, who published a book in 1832 called *Physiology of the Pear*; Goldstein notes that Peytel was "executed in 1839 for murdering two people, including his wife, but it was widely believed that the harshness of the penalty resulted from his book" (p. 132).

The book is nicely illustrated, including several before and after caricatures that show cuts insisted on by censors. Some of the caricatures are by the author's hero, André Gill, to whom the book is dedicated. It detracts only a little that the text is somewhat repetitive, and the commentaries on the illustrations repeat sizable chunks of the text. Some elegant vari-

ation would have helped as well: caricatures and their authors are invariably described as "brilliant" and their protests inevitably "bitter," probably true enough, but there are other adjectives.

If Goldstein is well read in the history of caricature, his summaries of each of the regimes are rather hit or miss, as are the placing of accents on French names (and the spelling of "Bourdeaux"; there is one in the Drôme, but that is not the one he means). In the discussion of the Boulanger affair, a significant turning point in the evolution of mass politics and political imagery in France, the author ignores important recent work on the subject.

Goldstein leaves for others, perhaps because he is primarily concerned with newspaper caricatures and images, what arguably are the two most important questions raised by his material. We need to know much more about how and to what extent caricatures were diffused in France by peddlers, hawkers, and other people and how they were read by the ordinary people they reached. Goldstein is off to a good start, and one hopes that he and others will carry such investigation forward.

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JACQUES RANCIÈRE. *The Nights of Labor: The Worker's Dream in Nineteenth-Century France*. Translated by JOHN DRURY. Foreword by DONALD REID. Philadelphia: Temple University Press. 1989. Pp. xxxvii, 442. \$34.95.

This book has already had an enormous impact on specialists in French labor history, and its availability in English will carry its message to a much wider audience. The translation is excellent, perfectly capturing Jacques Rancière's ironic (and sometimes sardonic) tone and rendering his colloquialisms into accessible English. The introduction by Donald Reid, whose grasp of contemporary French Marxist debate is remarkable, is almost worth the price of admission by itself.

Rancière is a philosopher whose early master was Louis Althusser. Like that of many of his young colleagues, his flirtation with Althusser was troubled and the breakup stormy. He abandoned structuralism in favor of an intellectual critical activism that mirrored workers' day-to-day resistance on the shop floor and in their modes of living and founded the influential journal *Les Révoltes logiques*. The step to the study of workers in the era when the class itself was being formed was then a short one. The result, after several articles and a marvelous collection of writings by workers (edited with Alain Faure), was *La Nuit des prolétaires*, published in 1981.

The book's main themes can be outlined rather simply, but there is no possibility, in a short review, to do justice to the rich, dense, and consistently stimu-

lating intellectual feast that the book actually provides. It is a study not of the working class in the mid-nineteenth century but of representations of the working class by writers whose origins and connections were in the world of manual labor. The title, slightly skewed by the too-cute translation, refers to the nights of these men and women and what they did after work, which was to read and to write. They did not want to be workers but writers and intellectuals, people of another class. They might write on behalf of the working class, present their prose to the middle-class reading public, and bring visions of working-class life and a different future into the discourse of the public sphere, but their published record cannot be viewed as the authentic *mentalité* of the working class. This crucial point, generated by the critical stance demanded by poststructuralist and deconstructionist thought, naturally gives pause to any historian who seeks to derive pictures of working-class consciousness from such writings. More fundamental still, we are forced to rethink cherished images of concepts such as skill. The writers for *L'Atelier* wanted to impress on their bourgeois admirers the tragedy of the collapse of unalienated, fulfilling work as a result of machines and vile "ready-made." Thus, images of golden ages, harmonious workshops, independent workers, and skilled artisans filled their pictures of the past—and their visions of the future. Rancière is at pains to point out how little this corresponded to the catch-as-catch-can character of most workers' lives in this era (Michael Sonenscher has demonstrated that such flux was hardly foreign to the eighteenth century either; see his *Work and Wages* [1989]). Indeed, workers in the most militant trades may have experienced poor and uncertain lives, but deskilling—the alienating loss of one's identity in work—seemed to have little to do with their discontent.

Nevertheless, the utopias that captured the imaginations of these workers-becoming-intellectuals had everything to do with the overcoming of alienation, whether in the form of fraternally associated worker cooperatives, phalansteries, or Icarian collectives. These dreams are the main subject matter of the book, and Rancière presents them in fascinating detail. But did they in fact constitute the actual proletarian vision? In a way, we can never know for certain, although Rancière sometimes writes as if they did. But, in reality, such images were conditioned by the circumstances of their creation—made by people intent on using their skills with the pen to change their own station in life and presented to a public both titillated by the tragedy of a besieged culture and enthralled by the vision of some peaceful restoration of it. One was untrue and the other was impossible, although both would serve unimagined purposes after the strange defeat of 1940. Meanwhile, thus titillated and enthralled, the nineteenth-century

bourgeoisie held fast, and proletarians sought to define their world and themselves realistically.

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BENJAMIN F. MARTIN. *Crime and Criminal Justice under the Third Republic: The Shame of Marianne*. Baton Rouge: Louisiana State University Press. 1990. Pp. xiii, 364. \$39.95.

Benjamin F. Martin finds the Dreyfus affair, in all of its ambiguity, to have been a fitting emblem for criminal justice in modern France. On the one hand, part of the public clamored for the ideal of the individual's absolute right to impartial judgment regardless of the political consequences. Yet the reality of the affair entailed botched police investigations, a biased judiciary, and heavy political pressures. Martin maintains that such realities constituted the prevailing features of criminal justice whereas efforts to approach the ideal were both rare and ineffectual.

Martin's study provides a most useful survey of the criminal justice system. Chapters on the police, court procedure, the magistracy, and the bar summarize the huge secondary literature and bring to bear neglected archival sources. Martin skillfully casts light on the obscure administrators who animated the juridical apparatus while political leaders ignored it or fruitlessly debated its shortcomings. The work breaks new ground by delving into budget allocations for justice and into career trajectories among the subordinate personnel. Martin makes excellent use of police officers' and judges' memoirs to illuminate the way prosecutions were handled.

The author's inferences from crime statistics reveal a France troubled by grand larceny, sexual molestation, and murder, although the idiosyncratic method of calculating "rates" leaves the strength of the demonstration in doubt. He finds the repressive apparatus designed to deal with these plagues conspicuously lacking in equity or effectiveness. Martin believes the judicial police (whose function was investigating crimes) to have been plodding and too dependent on miscreant informers, especially prostitutes. Martin even alleges that the police regularly resorted to beatings to extract confessions. The magistrates' concerns with patronage and political influence for the sake of career advancements made them incapable of an impartial evaluation of their cases. Outside pressures were all the more deleterious in that France's inquisitorial procedures gave great weight to the investigating magistrate's dispositions. Martin even identifies a decline in the moral authority of the bar: "The focus of the profession had shifted permanently . . . from the ideal of a richly cultured, highly ethical defense to acknowledgement that an acquittal was the goal, however guilty the accused" (p. 254). Surprisingly, his indictment nearly passes over in

silence the class biases at work in selecting people for prosecution.

Although Martin is surely correct to make much of the flaws, his case for pervasive failure is uneven. A good deal of the evidence concerning corruption is necessarily circumstantial or anecdotal. That so much of it relates to the Parisian situation does not instill confidence in nationwide generalizations. The most problematical aspect of the argument is the failure to construct convincing standards for evaluating criminal administration. Every democracy faces a chronic struggle to balance deterrence, mercy, freedom, efficiency, and expense. Although Martin does silence exalted claims on behalf of the French system, he does not convince that it was egregiously corrupt. After all, public confidence in criminal justice remained high when faith in other important institutions had decayed badly.

To be sure, the public confidence was less a matter of critical scrutiny than ignorance or passive respect for authority. Martin deserves much credit for exposing so systematically the structural features that distorted the rendering of justice. Third Republic historians miss much if they remain oblivious to the underside of the judicial process. Like a model investigating magistrate, Martin has done his research thoroughly and has constituted an imposing dossier. Much like the justices Martin criticizes, however, he has pushed ambiguous evidence beyond its limits to a predetermined conclusion.

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JEAN-FRANÇOIS SIRINELLI. *Intellectuels et passions françaises: Manifestes et pétitions au XX<sup>e</sup> siècle*. Paris: Fayard. 1990. Pp. 365. 120 fr.

CHRISTOPHE CHARLE. *Naissance des "intellectuels," 1880–1900*. (Le Sens commun.) Paris: Minuit. 1990. Pp. 271. 149 fr.

Few subjects appear more predestined to controversy than the history of French intellectuals; hence, it seems appropriate that these two books have so little in common. Jean-François Sirinelli takes the long view, examining the evolution of intellectual politics from the Dreyfus affair to the present; his method is traditional, dedicated to events, slow to judge, in search of balance and nuance. Christophe Charle focuses on a single moment, the emergence of intellectuals as a recognized category at the time of the Dreyfus agitation, and his method searches beneath events for the "pure social logic" that determines them and shapes their deeper meaning (p. 13).

In Sirinelli's book no single concern organizes the whole story, but close to its heart is the question of the extent, and the consequences, of the Left's recent hegemony in French intellectual life. The author argues that the image of a predominantly leftist intellectual community at the time of the Popular



Front is largely a myth, and he believes that the anticolonialism of the intellectuals at the time of the Algerian war has been exaggerated, too. In 1956—if no longer by 1960—it was still possible to claim the Dreyfusard legacy of republicanism and emancipation for the cause of *l'Algérie française* (see pp. 202–03). These nuances passed from the scene, as disillusionment with the Soviet Union encouraged the replacement of proletarian ideology with *tiersmondisme*, as the expansion of popular journalism and television sapped the power of intellectual debate in favor of more immediate and visceral images, and as the Vietnam War fueled a new leftism and a sometimes unthinking anti-Americanism; in combination, these developments pushed the intellectual Left to irrational and self-destructive positions that prepared the ground for the current crisis and apparent decline in the intellectuals' public position.

At its best Sirinelli's book is subtle and thought provoking, but its focus on the political surface keeps it from providing a wholly satisfying view of its subject. Doubtless the great events to which intellectuals responded were themselves responsible for some of the changes the author chronicles, but how did they interact with the intellectual community's internal development (its patterns of recruitment, shifting balance of disciplines, and so on)? Charle's implicit model of decline in intellectual influence from some supposed high point is also questionable; one might look instead for waves of waxing and waning influence (determined perhaps by the degree to which particular public issues become susceptible to evaluation in moral terms).

This last suggestion brings us closer to the very different perspective adopted by Charle. Following the model put forward by Pierre Bourdieu, Charle examines the emergence of the category "intellectuals" at the time of the Dreyfus agitation in terms of the interaction between and within the two "fields" of literary and intellectual life on the one hand and social power on the other. Each field is structured by the lines of force emanating from the two poles of "dominant" and "dominated" groups within it, and the relationship between the fields is also structured in terms of domination and subjection. From the point of view of society as a whole, intellectuals are in a relation of subjection to the more powerful and effective elites of politics and commerce, but some intellectuals are attached to those dominant elites while others are not, and, within the intellectual field itself, there exists a second relationship of "dominants" to "dominated." Every group or individual thus occupies a position that can be plotted by reference to the appropriate algebraic combination of *dominants* to *dominés*.

In Charle's analysis, the intellectuals who achieved self-consciousness at the time of the Dreyfus affair were attempting to close the gap between their purported place in national life and their actual position, between their claim to leadership as the heirs of the

literary tradition (and the representatives of the newer ideal of science) and the real situation of their "field," increasingly fragmented, invaded by larger numbers of aspirants, and economically weakened by the effects of depression in the publishing industry. In this situation, the new identity as "intellectuals" allowed those who assumed it to respond to their crisis of status by substituting a collective claim to moral authority for the more private one characteristic of earlier eras, asserting a new form of social power against the bourgeois elites who dominated them.

Charle is a thoughtful and acute analyst, and his book contains many stimulating observations and insights. His work reminds us that power relations are often operative where the self-consciousness of actors discourages us from looking for them, cautions us against accepting the Dreyfusards' claims to moral heroism uncritically, and provides much revealing information. All the same, the attempt to organize social action wholly in terms of power and domination ends up squeezing out other commitments and motivations. A common-sense view of why people in more "academic," less practical disciplines signed petitions more often than others might be that people who had chosen a life with more moral than material satisfactions were more likely to be attracted by a politics that highlighted moral issues. Charle's view is that "the dominated pole of the academic field, like the dominated pole of the literary field, was more drawn to this new form of public action because it was wholly excluded from other possible forms of power" (p. 187). That the representatives of more academic disciplines were in a sense "dominated" within universities (as they are still today) is a relevant point from which to consider their politics, but so is the basis—in personal identity, family background, psychological make-up—on which they made their original disciplinary choices. Charle seems to acknowledge such problems when he notes that actual situations need to be analyzed in terms of a whole series of "secondary factors" and that real individuals do not act precisely in the ways that theory predicts. But this still claims too much, because it implies that, in whatever degree the statistics show that individuals do behave as "pure social logic" would dictate, those individuals actually act for the reasons—for example, exclusion from other possible forms of power—that the theory proposes. Is this explanation for their actions really better than the ones they provide themselves?

Charle is correct that the purely moral account of intellectual politics sometimes offered at the time is inadequate and that the claims of the Dreyfusard intellectuals implied a "struggle over the legitimate definition of *dominants* and the mode of social domination" (p. 223). But only a narrow reading of the moral claims asserted by the Dreyfusard intellectuals justifies opposing this conclusion to their self-understanding. Their politics demanded that rational judg-



ment be recognized as a principle of modern social life, and it was this that made their "mode of social domination" contrast with their opponents' assumptions about the primacy of status and community. The claims people make about the meaning of their actions are seldom adequate, but nor are they irrelevant in the way implied when, as here, whatever does not fit the mold of a materialist theory is rejected as "idealism." Consciousness needs to be put into relation with the conditions of existence and experience in a given time and place, but the attempt to grasp all of human interaction in terms of the relations between *dominants* and *dominés* is too narrow a basis on which to ground this task.

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FRANCIS MCCOLLUM FEELEY. *Rebels with Causes: A Study of Revolutionary Syndicalist Culture among the French Primary School Teachers between 1880 and 1919*. (American University Studies, series ix, number 54.) New York: Peter Lang. 1989. Pp. xiv, 368. \$55.95.

The title of this book is a rebuttal to Peter Stearns's contention in *Revolutionary Syndicalism and French Labor* (1971) that pre-1914 revolutionary syndicalism in France was "a cause without rebels." Instead, Francis McCollum Feeley prefers (p. 6) the approach of Jacques Julliard, who "differentiated between the political term, 'anarcho-syndicalism' and the cultural label, 'revolutionary syndicalism'" and argued that the latter was "'more a matter of behavior than of doctrine'" (*Fernand Pelloutier et les origines du syndicalisme d'action directe* [1971]). Thus, Feeley asserts that revolutionary syndicalism was "above all a culture" whose "hallmark" was "defiance and above all skepticism" (p. 7).

This emphasis explains, in turn, why Feeley devotes the first half of his book to the ideological and material characteristics of the educational system reformed after 1879 by Jules Ferry and other founding fathers of the Third Republic and introduces the Fédération Nationale des Syndicats d'Instituteurs, created in 1906, only at the end of the fourth of seven chapters. Although this approach positions syndicalist teachers within a social and institutional context, it regrettably limits the discussion of their organization and leaders. Whereas Minister Ferry and Ferdinand-Edouard Buisson, Ferry's director of primary education, receive twenty-four pages of coverage, Marius Nègre, the first secretary general of the syndicalist teachers' federation, is cited only once within 230 pages of text. A six-page biographical sketch of Nègre is relegated to appendix 3, which also treats three other militant syndicalist teachers. Because of the focus on "rebels" in the book's title, its main body could have profited from the integration of much of the material in the appendixes.

Syndicalist teachers, who affiliated with the Con-

fédération Générale du Travail, composed less than 3 percent of France's 120,000 public primary school teachers on the eve of World War I, but Feeley believes that they better represented teachers' interests than the rival *amicales* to which a majority of teachers belonged and from which the syndicalists split after the *amicales'* fourth national congress in 1905. The author's largely uncritical admiration for the syndicalists leads him to various condemnations of their republican, socialist, and amicalist opponents, and some characterizations of the opposition seem overdrawn. For example, Feeley contends that the republican press failed to support teachers when conservative and clerical groups in many localities harassed them, particularly after the separation of church and state in 1905 (pp. 138–45). By showing that the republican press regularly depicted teachers as victimized by clericals, however, Feeley actually provides important evidence to undercut the generalization about its lack of support.

The principal merit of Feeley's study is the presentation of material from archival sources and the press. This research is most fully laid out in a lengthy chapter concerning syndicalist teachers during World War I and their enthusiastic response to the Russian revolution after the Bolsheviks assumed leadership of it. Missing from the bibliography, however, are Peter V. Meyers's important articles on pre-1914 French teachers, Judith Wishnia's thesis on state employees' struggle to unionize between 1884 and 1926, and Anne-Marie Sohn's study of syndicalist-feminist teachers from 1919 to 1935. Feeley cites Persis Hunt's thesis, "Revolutionary Syndicalism and Feminism among Teachers in France, 1900–1921" (1975), but ignores differences of opinion between male and female syndicalists. Finally, this book's abundant spelling, typographical, grammatical, and translation errors on at least 40 percent of its pages are regrettable. Georges Clemenceau, twice prime minister (*premier*) of France, is incorrectly identified as the "president" (pp. 5, 218). A subject to which an author is so passionately committed deserved better editing.

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ANNIE BENVENISTE. *Le Bosphore à la Roquette: La Communauté judéo-espagnole à Paris (1914–1940)*. Paris: L'Harmattan. 1989. Pp. 184.

Annie Benveniste notes in her opening chapter that she originally intended to write a straightforward history of the small Judeo-Spanish community in Paris from its inception in the early 1900s to its near disappearance during the Holocaust. She soon discovered, however, that the early record of Ottoman Jews in twentieth-century France could not be recovered because primary documents were lacking and surviving participants in the migration and settlement who were interviewed wove fanciful tales of their

experiences. In contrast, the period of the 1920s and 1930s yielded detailed information on the structures and institutions of the Paris community. In reassessing her approach, Benveniste chose to synthesize subjective and objective data into a broad study that blends literary criticism with social scientific analysis. The result is a strangely disjointed work that is part an examination of the invention of history and part historical investigation.

The primary focus of the first chapters of Benveniste's study is on the manner in which the present-day identity of Jewish immigrants from Greece, Bulgaria, and Turkey and their descendants is allegedly hidden behind a denial of the prewar past. A trained sociologist whose interests lie in urban life, interethnic relations, and responses to modernization, Benveniste argues that accounts by immigrants of their early experiences assume the form of individual adventure stories and fairy tales rather than historical chronicles of a community. In so doing, they become symbolic representations of the saga of the "wandering Jew" that reflect both the distance of time and the traumatic impact of the Holocaust on the survivors' collective memory. In emphasizing the striking similarities among the various individual narratives of departure from the Ottoman empire and settlement in France, Benveniste claims that, for survivors of the community, "[t]he prewar past assumes the form of a myth: a myth of a voyage without history, outside of History" (p. 40).

The second half of the book is devoted to an intensive examination of the socioeconomic structure of the interwar community. Drawing extensively on Clifford Geertz and Hildred Geertz and Lawrence Rosen's study of the Moroccan *mellah*, Benveniste emphasizes the role of economic and social structures rather than cultural and religious institutions in defining the subjective and objective reality of the community in the 1920s and 1930s. Among the issues that Benveniste discusses are elements of institutional and occupational continuity and discontinuity between the Jewish settlement in the Ottoman empire and in Paris, the relationship between social class and assimilationism, and the changing roles of women.

There is much that is useful and revealing in Benveniste's study, yet the work as a whole does not hold together. In large part, the problem seems to lie in Benveniste's relative unfamiliarity with the techniques of historical inquiry and analysis. Thus, the author's excursion into a textual analysis of individual immigrant accounts is interesting, but the reader is left wondering whether a more skillful interviewer might not have jogged the memory of survivors or at least gleaned useful historical information from their fanciful descriptions of their early experiences. More damaging, the author never explains exactly how and why the experience of the Holocaust destroyed the immigrants' collective memory and replaced it with myth. A more intensive discussion of the attitudes and behavior of Judeo-Spanish Jews in France during

and immediately after the Holocaust might have helped clarify the matter. So, too, would have an examination of the relationship between storytelling and historical memory in Judeo-Spanish culture.

The second part of the book is more focused, yet here, too, the lack of historical expertise weakens the author's argument. Benveniste seems more than willing to accept the testimony of individual immigrants that they were drawn to the community by economic opportunities rather than by cultural ties despite the fact that in earlier chapters she questions the historical accuracy of individual accounts of immigration and early settlement. Is this a convenient oversight on the author's part to help strengthen her thesis or simply sloppy methodology? More important, if unconscious socioeconomic factors were the unifying element in the interwar community, what exactly was the nature of the collective memory that allegedly was erased by the events of World War II?

Benveniste is to be commended for introducing us to a heretofore neglected community in modern French Jewish history. It remains for a trained historian, however, to re-create the community in its fullness through a sophisticated and subtle use of both primary sources and secondary works.

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LUIS GONZALEZ ANTON. *Las Cortes en la España del antiguo régimen*. (Institución Fernando el Católico Fundación Pública de la Excmá, number 1213.) Madrid: Institución Fernando el Católico. 1989. Pp. xv, 378.

In 1977 Julio Valdón Baroque republished Wladimiro Piskorski's classic study *Las Cortes de Castilla en el periodo de tránsito de la Edad Media a la moderna, 1188–1520*. In his brilliant introductory essay, where he reviewed the "recent historiography" on the Castilian Cortes, Valdón focused exclusively on the medieval period. His concentration was warranted; until that time Spanish parliamentary history was dominated by medievalists. In the last decade, however, there has been an explosion of interest in modern Spanish parliamentary history. Regrettably most of this recently published literature has been overlooked by Luis González Antón; consequently, there is an air of obsolescence to his study despite its revisionist character.

González Antón takes to task those nineteenth-century liberal and nationalist historians and their twentieth-century disciples who have forged the orthodox account of Spanish constitutional history. According to their accounts, medieval Spain was characterized by strong parliamentary traditions and by a constitutional regime that was protoliberal and protodemocratic.

In Castile, the triumph of royal absolutism between the fifteenth and seventeenth centuries destroyed this

political legacy, but in Navarre, and even more so in Aragon, Catalonia, and Valencia, the spirit of medieval contractualism survived well into the modern era. This contractualism, expressed mainly in the form of the provincial *fueros* (liberties), allowed "the people" in these kingdoms to protect themselves from the abuses of monarchical government. It also gave them a defense against Castilianization and the ability to maintain their distinctive national identities.

González Antón attacks this "simplistic" historical "mythology" on all counts. He emphasizes the inherent weakness of sixteenth- and seventeenth-century Spanish parliamentary institutions, not only in Castile but throughout the Spanish realms. Among other considerations he points out that the Cortes could only meet when summoned by the crown, that it had no separate existence, that it had no independent legislative authority, that its powers to secure redress were severely limited, and that it had an absolute obligation to vote supply.

By comparing and contrasting the Cortes of Castile, Navarre, Aragon, Catalonia, and Valencia, González Antón arrives at some startling conclusions. He establishes, for example, that the Cortes of Castile was the strongest and most progressive of these Spanish parliaments and the one most imbued with the spirit of contractualism. By contrast, he portrays the Cortes of the other kingdoms as weak and divided, as bastions of conservatism and defenders of feudal privilege, and as institutions far more motivated to protect the interests of their members than to pursue the common good.

The corrective González Antón provides to orthodox Spanish constitutional history, albeit overdrawn, is welcome. But it is by no means new. The orthodoxy he so vigorously attacks is already in full retreat. Nevertheless, the book makes a valuable contribution to the field. His detailed, comparative treatment of the parliaments of the separate kingdoms of *ancien régime* Spain makes it possible to comprehend more precisely their fundamental similarities and important differences. This type of comprehensive Spanish parliamentary history is long overdue, and it is to González Antón's credit that he has tackled such a difficult task. It is, therefore, all the more regrettable that his research has not kept pace with recent historiography, particularly that relating to the Cortes of Castile, where his treatment is often superficial and occasionally simply wrong.

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FRANCISCO VILLACORTA BAÑOS. *Profesionales y burócratas: Estado y poder corporativo en la España del siglo XX, 1890-1923*. (Estudios de historia contemporánea.) Madrid: Siglo Veintiuno. 1989. Pp. x, 537.

Francisco Villacorta Baños has undertaken to write the social history of the liberal professions in Spain in the nineteenth and early twentieth centuries, an important but neglected subject. After a general introduction, which includes an unnecessarily long—forty-five page—history of the Spanish bureaucracy in the nineteenth century, the author describes the activities of various professional groups within the current of regenerationism in the early twentieth century. This is followed by a chapter, which would have made more sense coming earlier, that offers a statistical analysis of the evolution of the various professions and their economic and social positions. Then comes a narrative account of the various corporatist movements in the period from 1914 to 1923. This two-hundred-forty-page chapter is the core of the book, which really has more to say about the political or institutional than the social history of the professions.

Villacorta Baños's main point is that, from about 1890 on, the members of the professions—and he deals with civil servants, lawyers, judges, notaries, teachers, doctors, and pharmacists—found their economic and social positions deteriorating as the Spanish economy became more developed and the state took on a larger role in national life. The growth of large corporations led to the creation of new work situations, with increasing numbers of professionals becoming salaried employees instead of independent practitioners. At the same time, the presence of larger numbers of qualified people produced increased competition and reduced incomes.

Professionals responded by turning to their colleges, organizations that had been created earlier in the nineteenth century to regulate day-to-day activities, to demand full control of the internal organizations of the professions, including the admission of new members as well as relations between the professions and the general political, economic, and social environment. These corporatist movements sought to defend the economic and social positions of the professionals involved, usually employing a rhetoric based on recovering the "dignity of the profession." These movements became especially forceful after 1914, a period marked by rapid inflation and more general social protest. Corporatism was, then, a response to "the breakdown of the liberal order of the nineteenth century," an adaptation to mass society (p. 331). In his conclusion the author suggests that the revitalized college system could be manipulated politically when the hierarchies of liberal Spain were challenged by the Second Republic.

Not all professions had the same experience, and not all reacted in exactly the same way. For example, among civil servants, teachers and those in basic administration had low salaries and few opportunities for promotion. They were also among the most active groups in making demands. By contrast, engineers, state lawyers, and university professors were much better off and more quiescent. The most interesting case is that of the pharmacists. With the emergence of

large pharmaceutical manufacturers, pharmacists found themselves reduced to the condition of shopkeepers. By the 1920s their demands for the preservation of professional dignity had led them into experiments with producer cooperatives and even limited companies. Because Villacorta Baños deals with such a large number of professional groups, he is able to point out such differences.

The author's research in tax records and especially professional journals is impressive and provides us with a wealth of information. Unfortunately, he does not use his material as effectively as he might. The book is far from being user friendly; it is thorough and highly detailed but also dense and difficult to read. Villacorta Baños has an argument to make, but nowhere is it clearly stated, so that the reader has to work to tease it out. This is a valuable, pathbreaking study, but inside this massive volume there is an excellent shorter book trying to get out.

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RICHARD GILLESPIE. *The Spanish Socialist Party: A History of Factionalism*. New York: Clarendon Press of Oxford University Press. 1989. Pp. xxii, 520.

Richard Gillespie has written a ground-breaking history of the Spanish Socialist party (PSOE) from the end of the Civil War in 1939 to the party's rise to power in 1982. In contrast to the abundant bibliography on the history of the party both before and during the Spanish Civil War, little has been written about the party's experiences during the Franco period. Gillespie has done an impressive job of assembling information from personal interviews, party and personal archives, periodicals, and scholarly publications.

After a brief introductory survey of the early years of Spanish socialism (1879–1939), Gillespie dedicates two lengthy chapters to the experience of Spanish Socialists in exile and inside Spain during the period from 1939 to 1970. During most of that period, the principal events occurred outside of Spain, but Gillespie also stresses the significance of the “men without names” who maintained a Socialist presence within Spain, especially in Asturias and the Basque province of Vizcaya and to a lesser degree in Madrid and Valencia. In Andalucía, which was later to play such a crucial role in the development of the PSOE, Socialist activity was minimal until well into the 1960s when a small group of young militants including Alfonso Guerra, Luis Yáñez, and Felipe González began to achieve some minor successes.

Gillespie traces and analyzes in detail the complex interactions among the various Socialist factions and groups, which led to the existence by 1974 of four main expressions of Spanish socialism. Over the following four years, the modern PSOE gradually estab-

lished its preeminence. The process of unification of the party, which Gillespie explores in detail, was accompanied by a dramatic shift in its political position. Under the leadership of González, the PSOE abandoned its radical positions and became one of the most moderate European Socialist parties.

Gillespie attributes the shift in the PSOE's position to both domestic and international factors. On the domestic side, he sees the threat created by the residual power of the old regime as a powerful stimulus to moderation, which was also encouraged by rising unemployment that weakened labor militancy and decreased the power of the unions, especially the Unión General del Trabajo (UGT). Equally important were the party's desire for electoral success and the rapid growth of membership, which multiplied tenfold in the late 1970s, bringing into the party many political novices who were responsive to González's leadership. In Gillespie's view, the international factors, particularly the influence of the West German Socialist party, although important in the increasing moderation of the PSOE, were less significant than domestic factors.

Gillespie announces in the preface that his main interest in writing the book lay “in the nature of the factionalism that has dominated the party's life” (p. v)—hence, the book's subtitle. Gillespie has undeniably succeeded in documenting—sometimes in rather wearying detail—the factional struggles of the PSOE. It is less clear that he has demonstrated that this factionalism had a positive side or that “it was through their internal struggles that the Socialists eventually managed to revise and modernize their outlook and activities” (p. vii). One need not, however, agree with Gillespie's thesis to appreciate his book as the best study to date of the PSOE in the Franco and post-Franco periods.

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KATHLEEN C. SCHWARTZMAN. *The Social Origins of Democratic Collapse: The First Portuguese Republic in the Global Economy*. (Studies in Historical Social Change.) Lawrence: University of Kansas. 1989. Pp. xx, 224. \$29.95.

This book is a revised doctoral dissertation in sociology that purports to be about the first Portuguese republic (1910–26), Western Europe's most unstable parliamentary regime in our century and the prelude to the same region's longest surviving dictatorship, the New State (1926–74). Although the volume presents little that is new or stimulating in historical analysis, it makes an arguable contribution to world systems theory and to economic dependency theory.

Historians who seek new materials based on Portuguese government records or on records related to Portugal from foreign governments will not find them here. The writing style is turgid, inflated, and



verbose, and more than a few of the quotations translated from the original Portuguese read awkwardly and seem unidiomatic. More serious with regard to the understanding of a complex phenomenon in a foreign context, key conceptual terms are mistranslated or poorly defined. The reader is bombarded with central words such as "democratic," "class," "capitalist," "liberalism," and "legitimacy" and given American or English meanings rather than Portuguese renderings with different meanings and nuances. A misunderstanding about the term "democratic," in fact, leads to a significant error: the author states that there were two separate parties, Democratic and Republican, whereas in fact there was only the Portuguese Republican party (PRP), whose unofficial nickname was the Democratic party. The word "democratic" acquired negative tones in common usage between 1910 and 1926, quite apart from the textbook definition of what constituted "democracy" in theory.

In the portion of the book that presents historical narratives of a sort, there are numerous factual errors and inaccurate generalizations. To cite only a few in chapter 1: *rotativismo*, a system whereby constitutional monarchist parties managed periodic changes in office, was never reduced to a predictable cycle each "two and half years" (p. 30); the Republican party leadership before the revolution of the 1910s was not, for the most part, drawn from Portugal's "landowning class" (p. 33); a treaty never gave the Delagoa Bay Railroad and the port of Lourenço Marques in Mozambique to South Africa (p. 31); and the Unionist Republican party was never known as "PUR" (*Partido União-Republicana*).

Although the author cannot be faulted for the book she did not write, this professional historian regrets that the social origins of Portuguese politics and policies were not profoundly analyzed and exposed. Although Schwartzman includes social origins along with world economic systems theory as major concerns, the reader will search in vain for definitive material on the social origins of Portuguese leadership, for example. In the largely secondary and tertiary materials used, biographical reference works now available to some degree were not employed. The biography of Portuguese leadership remains sadly unexplored.

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PHILIPPE RAXHON. *Le Révolution liégeoise de 1789 vue par les historiens belges (de 1805 à nos jours)*. (Études sur le XVIII<sup>e</sup> siècle, number 6.) Brussels: Université de Bruxelles. 1989. Pp. 199. 850 fr.

Over the last two hundred years, historians of the Liège revolution have singled out the unique character of their democratic revolt in the corner of what

would later become Belgium. On August 18, 1789, the "bon bourgeois," textile workers, and metallurgists of Verviers and Liège, impatient with the rule of their unpopular Prince Bishop and inspired by the revolutionary Parisians, stormed the Hôtel de Ville and conquered the Citadel of Liège. The nobility and clergy renounced their financial privileges while the Third Estate resuscitated and reformed a centuries-old constitution to guarantee their natural rights and national sovereignty. Philippe Raxhon explores the Belgian historiography of the Liège revolution.

The earliest historians emphasized the particularity of the revolution that they had just survived. These first chroniclers considered the Liège revolution to be an exclusively local rebellion with singularly nefarious ends. In contrast, Adolphe Borgnet and the next generation of Belgian historians embraced the moderation of their revolutionary forefathers who had struggled to secularize governmental power. Only within the last hundred years have historians begun to consider the Liège revolution as part of a revolutionary movement—for some of democracy and for others of tyranny—surging out from Paris.

Raxhon explains that the Liège revolution served liberal and Catholic historians as a battleground for reenacting their own political disputes throughout most of the nineteenth century. The Catholics portrayed the revolution as a terrible calamity that originated in France and devastated the two stable foundations of Liégeois society, the Catholic church and the guilds. In their turn, the liberals commemorated the origins of their democratic struggle for Belgian independence with centennial festivities in 1889.

In his history of the histories, Raxhon chronicles a progressive evolution in historical methodology culminating in the work of Henri Pirenne and his compatriot in Liège, Paul Harsin. These historians recognized for the first time that the Liège revolution "had causes, a development, consequences, and characteristics all its own to which historians were obliged to remain faithful" (p. 140). Harsin's definition of history as a search for truth seems to guide Raxhon's own historiographical evaluation of the Belgian histories of the Liège revolution.

Objectivity becomes especially difficult in the last chapter as Raxhon guides his readers into the midst of contemporary Belgian politics. For Pirenne and many of his immediate followers, the Liège revolution had represented "the boldly unitarist expression of his vision of Belgian history" (p. 89). But many historians writing after World War II see modern Belgium as an artificially fabricated, unnatural state. To justify their own regionalism, they glorify the radical Liège revolutionaries as the forerunners of their separate Walloon community. Other contemporary historians highlight the similarities between the Liège and the French revolutions to support their own appeals for a twentieth-century reunification of Liège with France.

The intense political debates that Raxhon convinc-



ingly explains have molded Belgian historiography over the last two centuries and have obviously influenced Raxhon's own perspective as well. Even the author's decision to include all Belgian historians rather than focusing on Liégeois historians of the Liège revolution is a politically charged choice in Belgium today. It should be added that Flemish historians writing in Dutch only make it into the footnotes.

Raxhon's examination of the overlapping influences of the Belgian political debates on Belgian histories of the Liège revolution suggests interesting comparisons with the more trenchantly divided French historiography of the French revolution. Unfortunately, however, because Raxhon assumes his readers' familiarity with the Liège revolution, he provides no narrative introduction. That may limit the audience of the book to Belgian historians, a small group indeed.

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B. W. DE VRIES. *From Pedlars to Textile Barons: The Economic Development of a Jewish Minority Group in the Netherlands*. New York: Koninklijke Nederlandse Akademie van Wetenschappen. 1989. Pp. 341. f. 95.

This thoroughly researched study of the role of Ashkenazi Jewish entrepreneurs in the inland textile districts of the Netherlands—principally Overijssel and North Brabant—is a valuable contribution to our knowledge of general Dutch social and economic history as well as general Jewish history and, specifically, the history of Dutch Jewry. Compared with England, Belgium, and some other Western European countries, the progress of the Netherlands toward modern industrialization was slow and hesitant despite the Netherlands' possession of a captive market in the shape of a large colonial empire, an advantage that, in the mid-nineteenth century, Belgium, the German states, and other competitors lacked. The most impressive progress achieved in the Netherlands during the early and mid-nineteenth century was in the textile sector, especially the manufacture of cotton fabrics for export to the Dutch East Indies. It is striking that precisely here, in the key sector in the Netherlands, a number of Jewish firms, from the 1830s on, played a considerable and, for a time, a preeminent role.

B. W. De Vries's book is impressive in that it places the crucial decades of transition, roughly the half-century from 1830 to 1880, in a broad historical perspective in respect to time, place, and social context. The first part of the study presents a clearly developed general analysis taking us right through the transition of Dutch Jews from poor pedlars struggling in increasingly adverse circumstances to make a living during the eighteenth century to successful manufacturers whose success and affluence were as-

sured by the late nineteenth. The second part of the book focuses especially on case studies that enable us to see how a very wide range of social, religious, and local, as well as purely economic, factors contributed to the emergence of this distinctive manufacturing elite in the eastern Netherlands. Perhaps the most important, as well as one of the most representative, of these Dutch Jewish textile dynasties was the family Salomonson. Commencing with Mozes Salomonson, originally a modest trader in linens who was born at Almelo, in Twente, around 1760, the family attained national importance in the 1830s when the firm set up by Mozes's younger sons, Godfried (Godschalk) and Hein Salomonson, rapidly emerged as the largest textile firm in the eastern Netherlands and the largest supplier of calicoes and other cotton fabrics to the Dutch East Indies.

De Vries, surely rightly, allows that certain Jewish characteristics inherited from the Jewish past—and not least the eighteenth-century legacy, namely, great tenacity in the face of extremely harsh commercial circumstances—played an indispensable part in this transition, but he stresses that it is equally important when analyzing the success of this or any particular Jewish entrepreneurial elite to recognize the decisive and overwhelming influence of the local context. In other words these families were the product of specifically Dutch circumstances, and this is just as significant in assessing their contribution to the process of industrialization, and evaluating their entrepreneurial techniques and attitudes, as is their Jewishness.

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JEANNINE E. OLSON. *Calvin and Social Welfare: Deacons and the Bourse française*. Cranbury, N.J.: Susquehanna University Press or Associated University Presses, Cranbury, N.J. 1989. Pp. 341. \$55.00.

Jeannine E. Olson has produced the first major work on Geneva's *Bourse française* since Henri Grandjean's study in 1927, and scholars will find it well worth the wait. The *Bourse* was a comprehensive charitable organization conceived by French religious refugees in the years preceding France's first war of religion (1562). It funneled contributions from wealthy émigrés already established in Geneva to the new arrivals, many of whom brought little more than the clothes they were wearing.

The academic cry of the sixteenth century was *ad fontes*, and we should be grateful that Olson has followed this tradition. She has returned to the sources and mined the rich deposits of Geneva's wonderfully preserved *Archives d'Etat* by the painstaking process of paleographic transcription. She has worked through a half-century of account books, diaconal meetings, and related *Bourse* material. Giv-

ing shape to this trove of information is no mean feat. What emerges is a picture of the daily functioning of the *Bourse*.

Perhaps her most important discovery is that the *Bourse* was used as a covert fund to further Protestant missionary activity in France, which was proscribed by the French kings. The Genevan Councils scrupulously avoided giving official aid for fear of provoking royal reprisals. Olson discovered large sums in the account books regularly channeled to Laurent de Normandie (nobleman and long-time friend of John Calvin), who turned to publishing during his Genevan exile. De Normandie used *Bourse* funds to hire colporteurs to smuggle Protestant religious materials across the mountain passes into France. These payments stopped with the beginning of hostilities, when the breakdown of central authority permitted indigenous printers to operate. Missionary ministers and their separated families were also maintained through special *Bourse* disbursements.

In this book there is much to like. Olson has done her homework well, and it shows. The work is meticulously referenced, especially from sources in the *Archives*. Her translations into English are literal; although they are clumsy, they give one a flavor of the era, as do the numerous illustrations that adorn the book. The literary style is engaging. At the same time, one senses the transcribed material is massive and unwieldy. Some minor items are mentioned in two or three different places, but to the same end. The conclusion is primarily a recapitulation of earlier chapters whose retelling does not really add anything. It also surveys analogous social welfare improvements in other cities. Yet all of these changes chronologically preceded the *Bourse* and thus seem to fit more logically in the chapter on the origins of the organization. Of any comparison, the most important should be a detailed analysis of the *Hôpital Général*, the *Bourse*'s Genevan neighbor, but this is lacking. Most historians believe Calvin borrowed his diaconal theology from Martin Bucer and its implementation from what he found already established in the *Hôpital Général*.

These are small criticisms, however, alongside what Olson has unearthed for historians. The wealth of material that she has transcribed should provide the basis for many future publications. Students of Calvin's Geneva await them eagerly.

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GÍSLI ÁGUST GUNNLAUGSSON. *Family and Household in Iceland, 1801–1930: Studies in the Relationship between Demographic and Socio-economic Development, Social Legislation, and Family and Household Structures*. (Studia Historica Upsaliensia, number 154.) Uppsala, Sweden: Uppsala University; distributed by Almqvist and Wiksell International, Stockholm. 1988. Pp. 189. 142 KR.

Because of precocious national censuses (the first dating from 1703), Iceland has played an important role in the lively international debate of the last decades over the development of family and household. Nordic scholars have been among the first to replace the scrutiny of interrelated economic factors conducted by a previous generation of economic historians with a more theoretical approach. The richness of the Scandinavian and, in particular, the Icelandic sources permits verification of theories formulated by scholars at the local as well as at the national level.

Gísli Águst Gunnlaugsson is the first to undertake a study of Icelandic demography over a long term. Well equipped for this task, he has mastered the secondary literature pertaining to Europe and the North to which he has applied published and unpublished sources from Iceland. His objective is to develop and test hypotheses that suggest connections during the nineteenth and twentieth centuries between social, economic, and demographic developments on the one hand and simultaneous social legislation on the other.

Nature and climate combine with the politics of contemporary leaders to make Iceland a near-perfect social laboratory for the modern historian. The effects of a devastating volcanic eruption in 1783 that killed 20 percent of the population had demographic consequences traceable in the censuses of 1801 and 1816. From these records the author treats the first half of the nineteenth century (chap. 2) by focusing on seven parishes chosen to represent different economic, social, and natural conditions in the country. Although all seven parishes existed at a subsistence level, Gunnlaugsson can show a difference in family and household structure between populations that relied exclusively on farming and animal husbandry and those of the coastal districts where fishing became increasingly important. All families tended to be of the nuclear type, but households were larger in the farming than in the fishing districts.

Population increased unhindered until the middle of the century when a saturation point was reached under existing agrarian conditions. Contrary to Malthusian theory, this situation did not result in an increased death rate. Following a model developed by D. B. Grigg, Gunnlaugsson looks for other signs of overpopulation and for measures aimed at alleviating the problem. Chapter 3 focuses on the national stage as politicians in Reykjavik attempted to control reproduction, particularly that of poor people. A problem faced by medieval lawmakers as well, overpopulation is endemic to a country where the people live under conditions as inhospitable as can be found anywhere on the globe. Property qualifications were increased for marriage, and poor relief became more difficult to obtain. The number of paupers continued to increase despite these precautions.

The remainder of the study turns to the application of these provisions in Hruni and Gardar, two of the

seven parishes that represented farming and fishing respectively. In the period 1845–80 (chap. 4), laws were largely obeyed, and the population increase was accommodated through a rise in the mean household size as more people became servants, many on a permanent basis. Increasingly, however, population pressure made it impossible for local communities to enforce the laws (chap. 5). Poor people settled in coastal communities where fishing could have provided a livelihood sooner if it had been permitted by the authorities. Gardar thus spawned the new village of Hafnarfjörður, almost entirely dependant on the sea. Fishing therefore became the channel through which industrialization finally entered the country around the turn of the century. Only at this moment did the number of paupers begin to decline, producing a total disappearance of this category by 1930.

In concluding, however, the author is not able to verify R. Wall's suggestion of a special "Scandinavian" (or, rather, "Nordic") household; instead, Gunnlaugsson confirms the view that the Icelandic family and household were variations of the Western European model. The book is equipped with useful charts and tables but lacks an index.

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VAL D. RUST. *The Democratic Tradition and the Evolution of Schooling in Norway*. (Contributions to the Study of Education, number 34.) New York: Greenwood. 1989. Pp. xiv, 333. \$42.95.

In 1814 Norwegians gained near autonomy in internal affairs as post-Napoleonic settlements transferred the governance of Norway to Sweden. Norwegians could now build their own bureaucracy, including the public school system described in Val D. Rust's book. Rust delineates the formation of government policies, their reform, and the institutional shape of the emerging and changing school system. It was a unique process, he suggests, because rather than changing society through school reforms, Norwegians changed their schools to better reflect the democratic nature of their society. Rust further suggests that the process was unusually effective because a broad spectrum of interested parties participated and careful study and experimentation preceded major changes.

In the first of six chronological sections, Rust describes Norwegian society and education in 1814. Few countries including Norway had an effective school system then, yet most adult Norwegians could read. Many had attended rural parish schools, urban poor schools, or had been taught at home by parents or tutors. Higher education for those who did not go abroad had been provided by a bewildering array of institutions or by less formal means. The quality of education depended largely on wealth and status.

Norway's school system, Rust contends, went

through four reform cycles after 1814. Sections two through five cover historical developments, and a sixth (the book's strongest) describes Norwegian education in the 1980s. Beginning with the Great Educational Commission and continuing through the 1880s, the first two reform cycles established as public policy the ideal of a permanent public primary school system common to all children. The disparity between rural and urban schools, however, remained. Reforms from 1920 to 1939 gave Norway's system its essential shape: seven years of primary and either three of practical or five years of academic, secondary education. The reforms begun during this third cycle continued through World War II, a time period often glossed over by historians of education. The fourth and last series of reforms completed, according to Rust, the goals of structural reformers. They addressed the central issues of student differences and gave the system its current form: seven-year common school, followed by an increasing range of choices of vocational, general, or academic courses in the same secondary school.

Books in English on Scandinavian topics appear infrequently and are always welcome. This one does, however, have some weaknesses, and it needs editing. Syntax is erroneous or awkward in places, and not all Norwegian terms are well translated. To give just two examples, Rust has parishes containing several parishes instead of several congregations (p. 37), and, although *Volksleute* may work in German, "the folk people" (p. 70) is an awkward phrase in English. Rust uses the so-called mobilization model, after discussing its limitations, and each section also presents the context for educational reform. His analysis here is sometimes weak, and the notes do not reflect the more recent literature. The history of Norwegian educational reform is unique and in some respects pathbreaking, and this book, despite its weaknesses, contributes to the telling of that story.

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ALLAN CARLSON. *The Swedish Experiment in Family Politics: The Myrdals and the Interwar Population Crisis*. New Brunswick, N.J.: Transaction. 1990. Pp. xx, 235. Cloth \$39.95, paper \$29.95.

This is the first work in English to examine fully the ideological content and political construction of Sweden's population policy of the 1930s since Alva Myrdal's *Nation and Family* was published in 1941. Allan Carlson asserts that Alva Myrdal and Gunnar Myrdal had a concealed radical feminist agenda to weaken the traditional family by directing its remaining functions to the state. By the 1970s most of their program in housing, maternity benefits, and taxation had "enhanced the economic power and autonomy of women at the expense of the economic utility of men" (p. 198). The Myrdals, to achieve their goal of freeing

more rational individuals from the bonds of family and locality, exaggerated the depopulation crisis and abused already weak demographic theories and jargon-ridden social science (pp. 192–97). “Short of revolution or war, our century can show no better example of the power of ideas and human will in altering the course of a nation” (p. xvii).

After examining twentieth-century Sweden’s declining marriage and birth rates, and the responses of Christians, racists, Marxists, and neo-Malthusians to them, the author depicts the Americanization of the Myrdals’ thought by urban sociology and child welfare institutes of the 1920s. The Myrdals’ utopian socialism and American psychology combined to produce the collectivist argument of *Crisis in the Population Question* (1934). Socialized consumption and public responsibility for maternal and child health became party priorities. With rich detail and clarity, Carlson relates the politics of population both in and outside of the Parliament of 1937. The influence of radical architects with whom the Myrdals worked to make housing a centerpiece of Social Democratic pronatalism is especially well depicted.

Yet for all of his valuable research, Carlson fails to support the contention that the Myrdals concealed their radical purposes or abused their statistics. Alva Myrdal’s proposed collective house was more openly feminist than any enacted program. Many of the couple’s ideas, such as protecting the employment rights of married women and supporting out-of-wedlock children, had a long history in the Swedish women’s movement. An open statement of values was central to Gunnar Myrdal’s conception of political economy, and he used the best numerical tools available in the 1930s. Carlson cites the Myrdals’ critics, not their evidence, to substantiate his charges. Finally, no government could guarantee that a stable or increasing population would follow the enactment of their program. Swedes created the material conditions so that couples who wanted children could afford them. Three healthy subsequent generations suggest that Carlson’s pessimism may stem not from the nation’s decline but from his own fear of feminism.

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KEES GISPEN. *New Profession, Old Order: Engineers and German Society, 1815–1914*. New York: Cambridge University Press. 1989. Pp. x, 357. \$44.50.

Kees Gispén’s book is an ambitious study of German engineers before 1914. He sets the stage appropriately with a discussion of Prussian bureaucratic disagreements over technical-vocational versus general-liberal arts curricula in schools founded for businessmen and technicians after 1815. The same dispute plagued the Association of German Engineers

(VDI), founded in 1856. The “school culture” of the engineering professors and teachers who led the VDI in its early decades clashed with the “shop culture” of business-oriented engineers who opposed the impracticality of theoretical science and higher education. Some engineers broke away to form their own societies in the 1880s. By 1900, the shop culture was dominant, annually graduating thousands of engineers from a variety of vocational schools. These “nonacademic” engineers often competed in the same overcrowded job market, however, with “diploma engineers” from Germany’s engineering universities (*technische Hochschulen*). The result was further division within the engineering profession. The Association of Industrial Technicians (Btüb) sought to organize engineers for trade union action, whereas the Association of German Diploma Engineers (VDDI) bitterly opposed this affront to engineers’ professionalism.

Gispén does not eschew use of theory for a strictly empirical approach. While the study is solidly researched—based primarily on rare engineering journals and numerous business archives—it is also guided by recent sociological models. He departs from Emile Durkheim’s ideal type of homogenous occupational communities with shared values for the emphasis that Rue Bucher and Anselm Strauss place on segmentation in a community or profession. Their theoretical construct posits professions “as loose amalgamations of segments pursuing different objectives in different manners and more or less delicately held together under a common name at a particular period in history” (p. 7). The facts that Gispén has unearthed fit into the Bucher-Strauss model quite well. The segments of the German engineering community were held together on the eve of World War I by common work, pride in expertise, and fear of the proletariat, but the profession’s balance was precarious given the different educational backgrounds, segmental self-images, and organizational agendas. Above all, however, it was the new profession’s alienation from an unresponsive old order that drove the supporters of the VDI, the Btüb, and the VDDI to the Nazis after 1918.

Gispén has performed yeoman’s service with his sociopolitical analysis of a significant profession. Moreover, he reveals the historical origins of the pro-Nazi sympathies of many of its members—“the purpose” of the book (p. 331). But a few criticisms can be offered. His assertion that school culture engineers attempted to ingratiate themselves with “quasi-aristocratic” preindustrial elites by requiring classical education before entry to top engineering institutes is misleading. The long-standing trend toward classical education was thoroughly bourgeois, albeit nonindustrial. The established aristocratic elites resisted this new curricular wave consistently after 1815. The practical bent to Prussian educational reforms in the late 1870s occurred, as Gispén observes, after Otto von Bismarck’s turn to the Conser-



vatives. Thus, a theory of quarreling middle-class factions similar to the Bucher-Strauss model, with one faction yielding partially to the culture of the other bourgeois groups, seems more consistent and appropriate. Gispén may be aware that classical education was a middle-class phenomenon, but his reliance on an established historiographical model that focuses on the persistence of Germany's preindustrial (that is, aristocratic) traditions before 1914 is ill-advised because it confuses issues.

An eclectic historian, Gispén also employs a competing "English" interpretation that views Nazism—and imperial Germany—as bourgeois or "industrial" in nature. Much of his in-depth analysis concerning relations at the engineering-business interface reinforces this school of thought, but unfortunately the book lacks a fully developed conclusion that elaborates his position on the "industrial-preindustrial" debate. The absence of an extended conclusion is especially problematic given the tendency of some of Gispén's evidence (for example, Prussian school policy after 1900, the Reich employee pension reform of 1911, and Reich patent law reform on the eve of the war) to depict the state as an autonomous force with an agenda nonidentical with that of big business—an interpretation that historians of the "preindustrial" thesis accept, but one that does not fit easily with the "English" school. In other words, the reader is left largely on his or her own to sort things out at the end of an extremely impressive exposition.

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MANFRED GAILUS. *Strasse und Brot: Sozialer Protest in den deutschen Staaten unter besonderer Berücksichtigung Preussens 1847–1849*. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 96.) Göttingen: Vandenhoeck und Ruprecht. 1990. Pp. 546. DM 112.

MICHAEL WETTENGEL. *Die Revolution von 1848/49 im Rhein-Main-Raum: Politische Vereine und Revolutionsalltag im Grossherzogtum Hessen, Herzogtum Nassau und in der Freien Stadt Frankfurt*. (Veröffentlichungen der Historischen Kommission für Nassau, number 49.) Wiesbaden: Historische Kommission für Nassau. 1989. Pp. ix, 662.

Social historians are slowly, belatedly, expanding our picture of the German revolutions of 1848 and 1849 by transferring attention to areas long neglected or ignored. But it cannot be said that they are clarifying that picture. On the contrary, they are uncovering new complexity in a landscape that was already crowded with enough detail for a variety of clashing interpretations. These two new examples, each from a different methodological perspective, show us a little of what was obscured by the traditional emphasis on political history and present some challenges to our customary interpretations.

Michael Wettengel's book is mainly a contribution to the history of *Vereine*, the voluntary associations that expanded rapidly from the late eighteenth century on and were a main vehicle for the creation of a "civil society" in Germany that eventually replaced the old corporate social order. Their growth in the hostile political environment of the early nineteenth century has been traced by historians such as Thomas Nipperdey, Otto Dann, Wolfgang Hardtwig, and many others. Wettengel here describes the explosion in *Verein* activities that occurred after the precipitate introduction of freedom of association in the German states in the spring of 1848. These activities were so comprehensive that they can almost (but not quite) be taken as synonymous with the social history of the revolution. In any event, they constitute a big enough slice of it to justify Wettengel's close attention to them. Among other things, this approach enables him to track the organized political activity of democrats, liberals, and conservatives, all carried on through *Vereine* of one sort or another. He identifies the regional conditions of terrain and communications that encouraged or facilitated such activities. He examines social and demographic influences on the various movements, with some interesting results: cities such as Frankfurt and Darmstadt were breeding grounds of moderate and conservative politics, whereas in many areas the rural countryside was the most reliable mainstay of the democrats. Small-holding farmers, not landless laborers, were among the staunchest supporters of the radicals, and they suspended their political activity only after serious coercion by the military, not when the governments tossed them a few concessions, as is frequently asserted. The *Arbeitervereine* were not forerunners of a modern labor movement but manifestations of a somewhat utopian faith in the potential of self-improvement through education: reading and singing were their chief activities, not organizing for strikes or revolution. Wettengel takes a strongly historicist stance by emphasizing the archaic nature of the society he is studying: it was still premodern and preindustrial in its social structure and social preconceptions, he says, and one must beware of presentist anticipations in interpreting phenomena such as labor unrest.

Manfred Gailus shares Wettengel's historicist sensibilities, but he directs his attention outside of or below the confines of formal institutions. His book is a contribution to the study of social protest in general, inaugurated less than two decades ago by historians such as Jürgen Bergmann, Heinrich Volkmann, and others in a conscious reception of the English Marxists E. P. Thompson, George Rudé, and Eric Hobsbawm. Gailus makes heavy use of "thick description" (another borrowing) of selected events to tease out of them a social or political meaning that might otherwise be passed over. The revolutionary years are of course rich enough in social protest actions, but Gailus redefines the era by including in it the hunger riots of 1847. The events he wants to consider, mainly



communal upheavals that pit masses against elites, are all of a piece from 1847 through 1849, he asserts. They are a form of "collective bargaining by riot," a premodern phenomenon in which the multitude attempts to coerce the authorities into doing the right thing. Behind these actions lies the notion of a "moral economy," one that justly provides everyone with the necessities. When this moral economy breaks down, as it did in the 1840s, the community riots until things are put in order. The old feudal elites, says Gailus, understood how to keep the lid on by timely intervention. They would, for example, alleviate critical food shortages, thus restoring a "moral economy from above." But in many localities in the 1840s the emerging new elite, progressive and capitalist, neglected such cumbersome and expensive social responsibilities and thus brought on public disorders that they could not control. This partly explains how riot turned to revolution in 1848. The multitude took over the public spaces, and old and new elites, normally competitors, were forced to work together to regain control of the streets and reestablish elite authority. That the new elite—the "bourgeoisie"—sided with the old elite in this crisis is neither surprising nor culpable, says Gailus, criticizing the officially Marxist historians of East Germany who label this action a betrayal of its historical mission, for the popular culture of 1848 was so alien to the "open society" envisioned by the new elite that a "popular front" strategy could not have functioned. The uncomfortable contacts of liberal *Volksfreunde* with the unruly *Volksmassen* illustrate that fundamental incompatibility.

By adopting the perspective of social history and emphasizing popular movements, both of these works challenge the hitherto settled notion that the events of 1848 and 1849 constitute a "bourgeois revolution"—Gailus by design, Wettengel more by implication. They suggest, instead, that it was an upheaval caused by the strains of incipient modernization, or of capitalism, on a traditional society. But how infinitely various are these strains: Gailus treats only the eastern provinces of Prussia because, he admits, his approach fits the conditions of that region but others not so well; Wettengel chronicles only the quite different region around the confluence of the Rhine and the Main, which with its small cities and its smallholding farming communities had a degree of social homogeneity that larger areas lacked. And each of these studies commendably points out the vast differences found within their small areas, which in turn contrast sharply with other German regions, such as the Württemberg communities studied by Wolfgang Kaschuba and Carola Lipp. Thus, the picture of the revolutions of 1848 and 1849 grows more interesting but at the same time more puzzling.

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WOLFRAM SIEMANN. *Gesellschaft im Aufbruch: Deutschland 1849–1871*. (Edition Suhrkamp, number 1537.) Frankfurt, a.M.: Suhrkamp. 1990. Pp. 354. DM 18.

Devised and edited by the Bielefeld historian Hans-Ulrich Wehler, the "Neue Historische Bibliothek" is a series whose volumes summarize and reinterpret the latest scholarship on specific historical topics. The modestly priced paperbacks are popular among German students, and they also attract occasional readers from the general public. English-speaking academics may find these works particularly useful for preparing and keeping up-to-date lectures in modern German and European history.

Wolfram Siemann, who wrote an excellent volume on the German revolution of 1848–49 for this series in 1985, now returns with a general history of economy, society, and politics in Central Europe during the two decades from 1849 to 1871. He notes that although the 1860s, the period of German national unification, have been the subject of intensive historical research, the previous ten years are the "least studied decade in the nineteenth century" (p. 12). His work involves a search for common themes uniting developments in the two decades.

One such theme is the long-term effects of the revolution of 1848–49, which Siemann believes set the framework for political life. As might be expected from the author of a lengthy monograph on the nineteenth-century German political police, Siemann convincingly details the reaction of the 1850s. His account of the police's ingenuity in undermining freedom of the press in spite of the abolition of prior censorship is a high point of the book. Siemann sees reaction against the 1848 revolution and the repression of it as dominating the politics of the 1850s, suggesting, for instance, that counterrevolutionary cooperation between Austria and Prussia outweighed their traditional diplomatic hostility.

The heritage of 1848 also underlines his discussion of the very different period of political movement in the following decade. Siemann perceives the issues and lines of division of the mid-century revolution as shaping the movement for national unity and the nascent German labor movement. He also views these past issues as the backdrop to the constitutional conflict in Prussia as well as the lesser-known parliamentary conflicts in the other German states, and, ultimately, to Otto von Bismarck's policy of confrontation with Austria and the creation of a Prussian-dominated German national state.

If continuity is the main theme of the author's discussion of politics, he perceives social and economic developments as characterized by a radical break with the past. Following the latest scholarship, he describes the post-1850 decades as the period of the decisive breakthrough of industrialization and urbanization. Siemann outlines the resulting transformation of class structure and briefly sketches some of its cultural consequences. He also has an interest-

ing section on the growth of and changes in municipal government as it confronted the challenge of rapid urbanization.

I find the author's two main interpretive themes a plausible way to understand the period, but he might have said more about their interaction. Politics and economics and society often go their separate ways in the book. Siemann does not inquire, for instance, how the transformation of class structure that he describes affected the founding of the labor movement, the development of political parties, or the movement for national unity. Even if politics were carried out largely in a framework created in 1848–49, the same political ideas in a new social and economic setting could have different implications. Such interactions might also help to explain the different political character of the decades of the 1850s and 1860s, which is mentioned but never entirely elucidated. These differing interpretive preferences of the reviewer in no way detract from the book, which lives up to the high standards of the series and provides an informative, readable, succinct, and comprehensive account of a critical period of German history.

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JOCHEN-CHRISTOPH KAISER. *Sozialer Protestantismus im 20. Jahrhundert: Beiträge zur Geschichte der Inneren Mission 1914–1945*. Munich: R. Oldenbourg. 1989. Pp. xi, 506.

The history of German Protestantism during the twentieth century has received much attention, not least because of the church's ambivalent posture toward the Weimar Republic and the bitter internal conflicts, known as the "Church Struggle," which divided it during the Third Reich. Yet the existing scholarship has focused almost exclusively on clerical leaders and their behavior toward the state. It has dealt only slightly with the role of Protestantism in the larger society, either in the parishes or in the numerous welfare and missionary associations that composed the Protestant social enterprise. Jochen-Christoph Kaiser's thoughtful work on the Inner Mission, the largest of the Protestant service organizations, is thus a welcome contribution. Not only does it analyze the internal debates and external interactions of an institution that extensively incorporated the energies of lay persons, but it also deepens the current discussion regarding the murky coexistence of consent and dissent in the Third Reich.

From its founding in the late 1840s, the Inner Mission's goals remained ambitious, encompassing welfare services, health care, and evangelization, in an effort to stem the rising secularism of German society. According to Kaiser, the Inner Mission's public influence actually increased after the outset of World War I because the expanding welfare state

incorporated rather than displaced its services. Even the Weimar Republic, whose revolutionary beginnings threatened the legal status and social role of the German churches, evolved congenially. Relevant Reich and Prussian ministries underwrote the unusual collaboration of denominational and other voluntary welfare agencies, including the Inner Mission, the Catholic Caritas, and the German Jewish Welfare Agency, as a counterweight to Social Democratic influence in communal welfare programs. The Inner Mission's status rose further in the first months of the Third Reich as its Catholic rival, Caritas, aroused the regime's suspicions as to its political reliability. Kaiser notes that the Inner Mission's pragmatism and relative autonomy from the Protestant church administration not only encouraged its collaboration with Weimar but also kept it largely free of the "Church Struggle's" divisions, although many of its adherents privately chose sides between the Confessing Church and the more Nazi-sympathizing German Christians.

Although Kaiser lends support to the more recent and more detailed view of German Protestantism during the 1920s—one that stresses its *Vernunftrepublikaner* cooperation with the republic—he must ultimately return to a familiar leitmotif: Protestant conservatism. Particularly riveting is his discussion of the Inner Mission's attitude toward the Third Reich's eugenics program. As the regime pressured its doctors and nurses to carry out the sterilization of the mentally and physically disabled, the Inner Mission's internal debates revealed a mixture of horror, resignation, confusion, and half-heartedness. Although it objected, often vehemently, to the compulsion explicit in the Third Reich's eugenics legislation, it nonetheless shared Nazi definitions of the public good. Most of the Inner Mission's membership believed that the use of public expenditures to support the disabled, retarded, and incurably ill was a luxury Germany could ill afford if it were to rise once again as a great power. Thus, encouraging the reproduction of the fittest was not only desirable but necessary. Kaiser is at pains to stress the extreme pressure and limited options with which the Inner Mission had to contend, including the constant threat of *Gleichschaltung*. He is therefore critical of ahistorical and retrospective judgments about what courses of action church-affiliated health care institutions should or should not have taken as the regime increasingly consolidated its hold. Yet the evidence Kaiser presents as to the Inner Mission's welcoming of Nazism as a Christian state that would bring about social harmony, and its conformity (however uneasy) to the Third Reich's assault against the genetically "inferior," makes one wonder whether his caution is necessary. His study makes at least as strong a case for the role of consent in the absence of an effective opposition to National Socialism as it does for the ubiquity of repression.

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UTE DANIEL. *Arbeiterfrauen in der Kriegsgesellschaft: Beruf, Familie und Politik im Ersten Weltkrieg*. (Kritische Studien zur Geschichtswissenschaft, number 84.) Göttingen: Vandenhoeck und Ruprecht. 1989. Pp. 397. DM 78.

In this exhaustively and meticulously researched study of wage-earning women in Germany during World War I, Ute Daniel has made a heroic effort to understand change in women's lives by attempting to synthesize Weberian structuralism, Lukacsian Marxist analysis, and gender analysis through the medium of *Alltagsgeschichte*, the history of daily life understood as a nub of interacting forces for social reproduction.

An extraordinary degree of military and civilian policing of public opinion generated valuable material about the subjective experience of wartime, which Daniel compares with a thorough description of objective conditions in order to evaluate the shift in her subjects' social construction of reality. She concretely shows how "war-weariness" worked to transform pre- or protopolitical dullness into effective antiwar politics. In addition, the author asks whether or not these wartime experiences emancipated wage-earning women.

Solid statistical work and careful attention to dense historical context support Daniel's conclusions that the war did not improve pay or work conditions but even worsened them in some ways. The most important change was in image: the war effort cast a positive light on women workers. By contrast, non-wage-earning wives of soldiers suffered an increasingly negative image as "undeserving" recipients of a nascent social policy, later elaborated into the "modern" welfare state. Both major employers, war industry and agriculture, however, preferred male workers from occupied territories or prisoners of war to women workers and did not teach the latter skills for postwar use.

Reproductive functions of the family changed because of the pressures of survival under conditions of severe shortages. Women shifted back to a preindustrial subsistence-oriented emphasis on home production. They lost some control over socialization, given the tendencies of male youths to rebelliousness, truancy, and delinquency before being drafted. Of course, women were blamed for this "asocial" behavior and for their own relaxation of the double standard in the absence of husbands, especially if they formed "unpatriotic" relationships with prisoners of war.

As shortages worsened, the government imposed more controls that women, the main food foragers, circumvented, thus gaining a reputation for undermining state legitimacy. They were also blamed for the growing mass demoralization over the long war, although their letters of complaint to the front were matched by the pessimistic battle reports of furloughed soldiers. But festering discontent was first expressed by working-class women and was protected

from violent retribution by traditional taboos. Their sense of entitlement had grown, paradoxically, with the failed efforts of the state to brake their progressive immiserization. Their protopolitical demonstrations to end the war, however, did not lead to more systemic social criticism or to female emancipation after the war.

This result will not surprise anyone. How could working-class women be emancipated if the whole of their class was not? Some middle-class women gained by becoming professionals in the welfare state that emerged to redistribute some social surplus as a way of forestalling revolution. But the women (and men) they monitored, although undoubtedly helped in some ways, neither gained nor were expected to achieve real autonomy.

The problem with Daniel's methodology and with her decision to focus on only two structures of society—urban working-class women and the state construed essentially as a bureaucracy—is that stasis becomes virtually a foregone conclusion. She privileges a narrowly focused structuralism at the expense of the conflictual relations of class (including between women) and of gender (including within the working class), both of which played important dynamic roles in official politics as well as in daily life. The dialectical relationship Daniel claims for her two structures fails to provide momentum for lasting change because she has uprooted them from deeper socio-economic relations of power. Thus, despite her truly dazzling scholarship, Daniel ends up ghettoizing her subject. Her paradigm can only offer a bleak perspective for women's emancipation.

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PETER D. STACHURA. *The Weimar Republic and the Younger Proletariat: An Economic and Social Analysis*. New York: St. Martin's. 1989. Pp. xii, 236. \$39.95.

Peter D. Stachura has made himself knowledgeable in the area of German youth and brings his knowledge to bear in this work. The content is interesting and the writing adequate to the task. The author has visited a large number of archives and has made use of their materials, although the great majority of his references are to secondary works, befitting a book of this scope.

Although anecdotal material would have aided his purpose, Stachura has managed to weave a story from laws, regulations, and descriptive statistics that conveys much of the atmosphere of the period. His judicious exploration of the topics concerning working (and nonworking) youth—the welfare system, the labor market, crime, politics—has enabled him to produce a sustained essay that somehow provides the reader with a sense of the rage and desperation of the young without a reporting on individual cases. By

neglecting to use life-history material, the work falls a bit short of what it might have been but is successful nonetheless.

Chapter after chapter outlines the promise of the Weimar welfare state, which might have tied youth to the republic, and details the subsequent undoing of that promise. Many factors hampered the humanitarian impulses of the republic, among them, debt imposed by the demands of the agrarian sector and the maintenance of overblown bureaucracies at every governmental level. The Socialist party and the trade unions were of little help; neither would consider the needs of the young at the expense of adult members. Youth were the first to be sacrificed. What was bad during the harder years of the republic became catastrophic when the depression twisted all structures beyond recognition. The appearance of teenage vagabondage, male and female prostitution, begging, suicide, youth gangs—all indicate that German society was failing to socialize its youth to work and to normal life patterns. Many of the working-class youth sought relief from their pain in radical politics, usually by opting for the Communist party but sometimes for the Nazi. Although the number of young people who switched from the Communist to the Nazi party was small, the ability to do so at all is taken by Stachura as a sign of the misery and the lack of ideological commitment among these young people.

Although sympathetic to the efforts of the Weimar republic, Stachura does not leave the reader with the impression that even the best of human services systems could have saved Germany from political chaos. The Great Depression crushed everything in its path, and this account reminds us how little defense we have against such massive, impersonal forces.

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ADOLF M. BIRKE. *Nation ohne Haus: Deutschland 1945–1961*. (Die Deutschen und ihre Nation.) Berlin: Siedler. 1989. Pp. 540. DM 98.

Adolf M. Birke, the author of this twelfth volume of the publisher's lavishly illustrated series on German history since Roman times, has written a useful account of the eventful decade and a half following World War II. Drawing on the burgeoning secondary literature and extensive published sources, Birke has, in contrast to most German scholars of the postwar period, covered the development of both West and East Germany. Readers who want more detailed information for the Federal Republic will still refer to the five-volume *Geschichte der Bundesrepublik Deutschland* (1983–87), written by a team of West German historians under the general editorship of Karl Dietrich Bracher, and, for the German Democratic Republic, to Hermann Weber's *Geschichte der DDR*

(1985). But, as a manageable overview of both West and East, Birke's judicious analysis should find wide use.

Unsurprisingly, Birke's overall assessment of the Federal Republic is positive, although not wholly uncritical, whereas his verdict on the now defunct German Democratic Republic is decidedly negative. The dominant figure of the volume is Konrad Adenauer, first chancellor of the Federal Republic. Like many others who have scrutinized that provincial Rhenish politician's late entrance on the stage of world history, Birke is obviously impressed by his ability in the eighth decade of his life to think clearly in a long-term fashion about the resolution of his defeated country's division and to act decisively in pursuit of his policies.

If the book had been completed only a few months later, after the collapse of the East German regime in the autumn of 1989, Birke could have pointed out that the recent turn of events amounted to ultimate vindication of Adenauer's insistence on according priority to establishing a Western-oriented liberal democracy in West Germany as opposed to pursuing an early reunification through compromise with the Soviets and their East German client regime. Just as Adenauer predicted, the West German republic that he was so instrumental in shaping eventually proved—two decades after his death—to be an irresistible magnet to the communist-ruled eastern part of Germany.

Some of the most informative parts of Birke's book are devoted to the foreign relations of West Germany. From his extensive scrutiny of Adenauer's tenacious struggle to wrest from the Western occupying powers the concessions necessary to establish the sovereignty of the Federal Republic, it becomes obvious that, with a less resolute and resourceful politician at its helm, West Germany might have lingered much longer in the limbo between occupied territory and independence.

Birke's examination of the diplomacy of the late 1950s and early 1960s is especially informative. At that time the United States, formerly West Germany's most vigilant defender against attempts at communist encroachment, began to show weakening resolve in the face of Soviet leader Nikita Khrushchev's repeated threats to the freedom of West Berlin. To Adenauer's distress, the United States several times gave every indication of willingness to make concessions on Berlin that would have fatally weakened the Western presence in that key city in the midst of East Germany. Only staunch resistance to such concessions by President Charles de Gaulle of France, whose confidence Adenauer succeeding in winning, brought the Western powers successfully to call Khrushchev's bluff on Berlin.

The book ends with the construction of the Berlin Wall in August 1961, which Birke, like most writers of postwar German history, takes as the completion of the country's division. In an illuminating discussion



of the wall's impact on West Germany, he contends that the passive response of the Western powers revealed to perceptive West German leaders that the apparent identity of their aims and those of the Western powers in the cold war had become illusory. As a consequence of the nuclear stalemate, the Western powers and the Soviets had come to respect each other's spheres of influence in a Europe whose division ran through the middle of Germany. As the acquiescence of the Western powers to the wall revealed, their repeated verbal commitments to the reunification of Germany had lost their force.

Birke points out that one of the first to realize the implications of this new situation was Willy Brandt, who as lord mayor of West Berlin had been bitterly disappointed in August 1961 by the failure of the Western powers to thwart the wall's construction. That experience, Birke persuasively argues, set in motion the fundamental reassessment of West German foreign policy by the leadership of the Social Democratic party that produced the bold new *Ostpolitik* adopted by the cabinet formed by Brandt in 1969, including the ultimately successful policy of *Wandel durch Annäherung* toward East Germany.

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RICHARD J. EVANS. *In Hitler's Shadow: West German Historians and the Attempt to Escape from the Nazi Past*. New York: Pantheon Books. 1989. Pp. x, 196. \$18.95.

As Richard J. Evans readily acknowledges, this is an essay in interpretation and synthesis, not a piece of original research. Evans also writes that the book is not a polemical work, but that claim may be open to question.

In one sense this is another book on the West German *Historikerstreit*, and much of the information will be familiar to those who have followed that debate. Evans, however, offers a unique perspective and a provocative thesis. To some extent following Hans-Ulrich Wehler, Evans sees the *Historikerstreit* as a battle over Germany's political future. He accuses a group of conservative West German historians of attempting a reinterpretation of German history in order to reestablish Bismarckian authoritarianism in a reunited East and West Germany. There is no doubt that the book makes some important contributions to the historiography. It provides lucid and sometimes very telling criticisms of the positions taken by the "conservatives" in the debate, especially Ernst Nolte's inadmissible use of "comparative atrocities." (Evans is much less detailed on the "liberals," whose point of view he clearly shares.) The author also makes an eloquent plea for the historian's need to judge peoples and events, and, although Evans is less explicit on this point, his book is a passionate brief for maintaining the *Sonderweg* thesis of modern German history. Evans remains convinced that Nazism

and the Third Reich was the logical, if not inevitable, culmination of German history.

All of this is useful and historiographically interesting, but Evans's major intent is to show the link between the position of the conservative camp in the *Historikerstreit*, the political agenda of the present German government, and the right wing of the ruling Christian Democrats. In this respect, I think the book is rather less convincing. Evans's conceptual tools are too heavy-handed. The label "conservative historians" is far too broad. Evans puts such different writers as Nolte, Hellmut Diwald, Andreas Hillgruber, and Michael Stürmer in this group, ignoring not only their different positions on a number of issues but also the vast qualitative differences in the writings of, say, Hillgruber and Diwald. The author also singles out Nolte as representative of the entire camp, which is certainly not the case. Important work by some of the "moderates," such as the late Martin Broszat and Eberhard Jäckel, gets little attention. The claim that virtually all of West German historical scholarship has a political agenda and that only the "liberals" have done any useful work leads Evans to unusual emphases. According to the author, before 1960, West German historical writing was historicism; only after 1960, social science. The debate between functionalists and intentionalists is largely subsumed under the heading of political differences.

In addition, the views of some conservative historians are distorted, and too much influence is ascribed to them in contemporary affairs. The conservative historians are blamed for everything from present-day Austria's "stifling corporatism" to what Evans calls the crisis in German political life. I, for one, cannot accept the thesis that scholars such as Stürmer and Hillgruber are (or were, in Hillgruber's case) longing to destroy German parliamentary democracy to reestablish Bismarckian authoritarianism. Finally, there is the very real problem of what Michael Wolffsohn has called *Geschichtspolitik*. The manuscript for the book was completed when the division of Europe into two blocs still seemed secure, and Evans uses the *Sonderweg* thesis to argue that the post-1945 division of Germany and Europe was historically justified. He is concerned that, if the campaign by the "conservatives" to "historicize" the Third Reich succeeds, it might lead to the reunification of Germany and consequently the renewed destabilization of Europe. As Evans writes, "If the relativization of Auschwitz succeeds, an important obstacle to the campaign for German reunification is . . . removed."

Events have indeed changed the face of Central Europe, but the dynamic forces in this transformation were the desire for freedom by the East German and East European peoples, not the arguments by one side in the *Historikerstreit*. Sometimes history does ignore historians.

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RONA GOFFEN. *Giovanni Bellini*. New Haven: Yale University Press. 1989. Pp. ix, 347. \$60.00.

Giovanni Bellini, also affectionately known as Giambellino, stands among a select handful of the most distinguished painters of the Italian Renaissance. He was the son of a painter, the brother of another, and the brother-in-law of still another, all of whom had a considerable place in the evolution of Venetian painting. As is widely recognized, Bellini, more than Andrea Mantegna who married his sister, was the effective founder of the Venetian School, and to one extent or another he gave rise to the careers of Sebastiano del Piombo, Giorgione del Castelfranco, and Titian. He had a long illustrious career that lasted some seven decades during which he produced hundreds, if not more, works of art. The period in which he flourished, the second half of the *Quattrocento*, until 1516, the year of his death, coincided with an outpouring of artistic creativity that culminated in the so-called High Renaissance. Unlike most of his contemporaries, including Mantegna, Bellini traveled hardly at all from his native Venice and apparently never even visited Florence and Rome, the leading Italian art centers of the time. His experience with the mainstream of progressive art was limited largely to examples of works located in his own city and in the nearby area, including Pesaro where he had a major commission. In other words, considering his preeminence, the variety of his output, and the fact that the details of his biography and of his artistic commissions are very poorly documented, the task of the art scholar in producing a monograph is particularly arduous. Rona Goffen's study of Bellini is an ambitious undertaking for which she has earned our admiration.

The precise year of Giovanni's birth has been the subject of critical discussion, and Goffen, who in my view correctly seeks to place it later than is usually given, offers circa 1433 and circa 1434 as well as circa 1433–35, which is a little frustrating for the attentive reader. Perhaps an even later date is called for, given the recent, convincing proposal for Mantegna's date of birth. The effective result of these postdatings is to eliminate some of the problems connected with the very early periods of these two artists by simply denying them. It may seem shocking, but the first firmly dated painting by Bellini is 1487, when he was at least fifty years old, raising a knotty, virtually irresolvable problem for a student of Bellini's evolution. This is not, however, an issue of particular interest for Goffen, nor are those of connoisseurship, despite the huge number of works that need to be catalogued. Among those pictures that go under Bellini's name are endless Madonna-and-Child paintings, which were destined for private devotion. They include autograph works, replicas, studio versions, imitations, copies, and outright forgeries, notwithstanding prominently located signatures. Even the pure works by Bellini, among the first to have been

painted in oil rather than tempera, have suffered extensively from enthusiastic cleanings and repaintings.

The strength of Goffen's book, whose illustrations are usually adequate, lies in the enthusiasm and respect that the author reveals for the artist and in her often moving and always pondered descriptions of the religious paintings. She seeks to provide the appropriate theological texts, often with a Venetian filtration, in order to explain the narrative qualities of the private devotional pictures as well as the mighty altarpieces, among the finest works ever painted. Think of the San Giobbe, the Frari, and the stupendous San Zaccaria altarpieces, not to mention the *Coronation of the Virgin* still in Pesaro. Goffen gives the secular paintings, which include a group of portraits, much less space, but her division properly reflects the entire *oeuvre*. One tends to forget that most of the paintings from the Renaissance were, in fact, religious in subject matter, and in the case of Giambellino, this was so, but his work also mirrored his own deeply felt religious convictions, which are very well translated by Goffen.

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LESTER A. SEGAL. *Historical Consciousness and Religious Tradition in Azariah de' Rossi's Me'or 'Einayim*. Philadelphia: Jewish Publication Society. 1989. Pp. x, 194. \$22.95.

By common estimation, Azariah de' Rossi (ca. 1511–ca. 1578) is considered the most erudite and critical scholar of sixteenth-century Italian Jewish culture. In his *Me'or 'Einayim* ("Enlightenment of the Eyes"), first published in Mantua in 1573, he used an impressive array of rabbinic and classical and contemporary Latin and Italian sources to explore such recondite areas as rabbinic chronology, the antiquity of the Hebrew and Aramaic languages, and the general reliability of rabbinic homilies. His frequent citations from Philo, Flavius Josephus, and even Christian religious authorities to argue his points represented a dramatic shift from previous Jewish usage.

Although considered a work of special interest by nineteenth-century scholars, de' Rossi's masterpiece has been relatively neglected in this century with the exception of S. W. Baron's pioneering essays written some fifty years ago. With the outpouring of recent scholarship on Italian Jewish culture in the last several years, the *Me'or 'Einayim* has finally received the attention it so fittingly deserves. Reuven Bonfil has offered an important new interpretation of the book; Bezalel Safran and Joanna Weinberg have devoted dissertations to the work. Weinberg has also produced several insightful essays and is presently completing an annotated English translation of this Hebrew classic.

Lester Segal's own contribution to recent de' Rossi

studies is a welcome addition to this new scholarship. Segal's book does not purport to be an exhaustive study of de' Rossi. Rather, it more narrowly focuses on de' Rossi's concern for establishing historical accuracy and the challenge this quest presented to upholding the sanctified authority of the rabbinic tradition in the sixteenth century. Through a series of short chapters accompanied with lengthy annotation, Segal fashions de' Rossi as a seeker of the truth in an age permeated with mysticism, pietism, and messianic anticipation, all militating against a positive Jewish reception to Azariah's quest. Segal is particularly successful in treating the work within the context of rabbinic and medieval Jewish culture. His analysis of de' Rossi's contemporary, the Maharal of Prague, is especially well done.

The work, however, is not without serious shortcomings. Segal discusses at length the goals of de' Rossi's scholarly enterprise but has little to contribute about the enterprise itself, the contemporary Latin and Italian sources that inform de' Rossi's work, and his actual place within the world of sixteenth-century Christian scholarship. Segal's perspective has much in common with that of nineteenth-century scholars who sang the praises of the enlightened de' Rossi seemingly threatening the parochial and obscurantist concerns of most of his coreligionists. Yet this view has been modified significantly by recent scholarship in reassessing both the supposed novelty of de' Rossi's achievement and the cultural ambience of Judaism in his day. As Weinberg has recently argued, de' Rossi was heavily dependent on the scholarly Christian world, and, when compared with some of his Christian colleagues, he was neither as bold nor as original as was once thought. Besides some general and superficial references to the historical writing of de' Rossi's actual intellectual attainments and limitations, Segal's portrait of Jewish intellectual life in Counter Reformation Italy is not compelling. To my mind, Segal underestimates the intellectual achievements of de' Rossi's Jewish contemporaries. Intellectuals such as Abraham Portaleone, Judah Moscato, David Provencal, Abraham Yagel, and others produced works of comparable magnitude and erudition and could appreciate fully de' Rossi's effort. And even mystics like Menahem Azariah da Fano, Mordecai Dato, and others were not totally closed to the kind of scholarship de' Rossi produced. The contrast between de' Rossi's brilliance and his contemporaries' reactionary and pietistic responses is overdone and fails to capture the highly complex cultural world that produced a de' Rossi. Whether or not the contemporary debate over de' Rossi's book was no more than "a tempest in a teapot," as Bonfil has argued, Segal still fails to recognize that many of de' Rossi's coreligionists actually lived comfortably with and made use of his scholarship.

In sum, Segal has tackled a challenging subject with occasional insight and sound scholarship. His book

remains limited, however, in presenting its protagonist as too solitary a figure, isolated from the scholarly world of his Jewish and Christian contemporaries.

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MARIO ASSENNATO. *Eroi della trasformazione agricola del mezzogiorno tra Settecento e Ottocento*. In two volumes. (Istituto per la storia del risorgimento italiano, numbers 76 and 77.) Rome: Istituto per la Storia del Risorgimento Italiano. 1989. Pp. 511; 513–1175.

Since the 1950s, the historiography of the *mezzogiorno* has moved from detailed analysis of eighteenth-century *illuministi* critiques of social and economic issues either to studies on class conflict, peasant uprisings, and landholding questions or to attempts at integration of *illuministi* perceptions with economic realities and social behavior during the transition from the subsistence economy of the eighteenth century. This two-volume study, meticulously researched and luxuriant in documentation of the Apulia region, belongs in the latter category. Informing Mario Assennato's presentation is his perception of the "virus" of inertia that permeated those powerful landowners, of both nobility and *borghesia*, who controlled the economic transformation without changing the social order. Thus, the "failed" revolutions of 1799, 1820, 1848, and even 1860 never produced a "Jacobin alliance" because the middle classes seemed as firmly entrenched in the social, economic, and political order as the nobility. Rather than seeing the initial revolution of 1799, to which much of volume 2 is devoted, as a "failed" or a "betrayed" revolution, Assennato views it as a *rivoluzione ripudiata*, in which the peasantry were delivered into the hands of the church.

The element of inertia is seen in the reluctance of Apulian landowners to refine olive oil (until 1830) despite the demands for it. During a lucrative period in the eighteenth century, Barese merchants took the initiative in seeking direct trade in Barese ships with Trieste and Ferrara and shipped unrefined olive oil to England and France. But commerce in finer varieties was left to the French and other Italian regions, and thus a true international market eluded Apulian oil. Economic transition was equally inhibited by political and social structures. For Assennato, the Bourbon kingdom was less a true state than a collection of provinces suspicious of central government interference and tied to a parasitic capital by taxes and regulations protective of Naples's consumer needs (vol. 1, p. 399). The Bourbon court could take the initiative as it did in 1782 in encouraging Giuseppe Maria Galanti, a utilitarian advocating the greatest happiness of the greatest number, to investigate the problems in the provinces, and this led to a series of detailed studies. Fears (if not inertia), however, prevented vested interests at the court from

accepting the reforms, such as one on duties suggested by the reformer and finance minister Giuseppe Palmieri (1789–93). Moreover, Palmieri himself, although interested in extending credit to the provinces, supported the assignment of land (gleaned from the church or domain lands reverting to the state) to large landowners from whom he hoped an ensuing entrepreneurial spirit would emerge, and he advocated ridding the land of civic uses essential to the subsistence of the peasantry.

The profile that Assennato presents of the extensive economic and social control of the large landowners is supported throughout the volumes, from their role in Molfetta in impeding both renters and small proprietors in the freedom of determining the harvest to their practice of usury (which extended to the professional middle class as well), from their avidity in acquiring church lands to their ability as usurpers of land to impede owner verification by communal administration. For the feudal nobility, the abolition of feudal privileges and jurisdiction during the Napoleonic period did not bring ruin because, in addition to legal acquisition of lands, their usurpations (under a distorted guise of *laissez faire*) had become institutionalized long before the French occupation. The author sees the transition from a subsistence to a commercial economy as brought about on the backs of the peasantry whose demands for social reform and justice eluded many of the innovative reformers. Throughout the nineteenth and twentieth centuries, the loss of civic uses remained a source of unrest, and cases of land usurpation flooded communal bureaucracy. There is a sense of continuity that permeates these two volumes, from the description of the deforestation that accompanied the expansion of grain production after the famine of 1764 to the aggressive fascist “battle of grain” of the 1920s and the postfascist resolution of land cases (one such case, begun in 1814, was not settled until 1985).

Assennato's command of both archival sources and secondary works is exemplary, seemingly exhaustive, and especially apparent in the textual analysis of those sources in the second volume where the events of 1799 as reflected in Andria and neighboring Gioia del Colle and Noci serve to support his thesis of the repudiated revolution. But such textual data reveal not only the complexities of social behavior and attitudes of the *mezzogiorno* during its economic transformation within traditional political and social structures but also suggest the reasons for the *attendismo* (the “wait-and-see” policy, if not inertia) of the southern *borghesia* in 1860. These two volumes along with the recent excellent socioeconomic study by John Marino of early modern Foggia are admirable additions to the expanding library on the *mezzogiorno*.

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FRANK J. COPPA. *Cardinal Giacomo Antonelli and Papal Politics in European Affairs*. New York: State University of New York Press. 1990. Pp. viii, 299.

This is the first scholarly book in English on Giacomo Antonelli, the chief minister and advisor of Pope Pius IX. As cardinal secretary of state from 1848 to 1876, Antonelli served at a critical period in papal and European history. These years witnessed the military defeat of Austria, Russia, and France, the unification of Italy and Germany, the emergence of the Roman Question, and the proclamation of the dogma of papal infallibility. According to Frank J. Coppa, the astute intellect and diplomacy of Antonelli enabled the Holy See to survive the crises engendered by these events.

Based on archival sources, including the recently opened Vatican archives on the pontificate of Pius IX, Coppa's book is a solid contribution to the history of the nineteenth century, with a focus on the Papal States. The purpose of the study is a more balanced presentation of Antonelli than has previously appeared. Coppa wishes to show that Antonelli did not dominate Pio Nono but was his faithful executor. Pragmatic and introverted, Antonelli was the opposite of the emotional and extroverted sovereign whom he served.

Consisting of fifteen chapters, the early part of the book traces the rise of the Antonelli family, a middle-class family that originated in the southern part of the Papal States. The wealth amassed by the family led later to the allegation that Giacomo used his position in the pontifical government to enrich himself, a charge that Coppa questions. The details that Coppa provides concerning the family, particularly Giacomo, who became a cardinal but was never ordained a priest, are engrossing.

In Coppa's interpretation, Antonelli the statesman emerges as a technocrat rather than an ideologue. In entering the government of Pius IX, Antonelli jettisoned his earlier conservatism and adopted the moderate conservatism that characterized his sovereign from 1846 to 1848. When Pio Nono turned against liberalism and nationalism, so did Antonelli. As the administrator of the Papal States, he rejected all reforms that might have undermined papal authority. On the other hand, he was willing to accept changes that promoted efficiency. Although there were occasional differences of opinion between Pius IX and Antonelli, the pope trusted Antonelli and never seriously considered replacing him. With respect to the dogma of papal infallibility, Coppa contends that Antonelli's lack of enthusiasm for a public pronouncement stemmed not from theological reservations but from a belief that it was unnecessary and inopportune.

In 1870, when Rome confronted troops of Victor Emmanuel II after the withdrawal of French troops, Antonelli decisively opposed any attempt at resistance. He joined Pio Nono as a fellow prisoner of the

Vatican and made the best of the situation. In the face of mounting criticism and bad health, he continued to execute the policies of the pope.

Coppa concludes his well-documented study with the statement that "Antonelli was neither a saint nor a sinner, neither a Richelieu nor Mazarin, but a hard-working pragmatic minister who was devoted to his sovereign's cause" (p. 196). I concur. The documentation is very impressive, taking up about one-third of the book. The only negative thing that can be said about the volume is that there are too many typographical errors; these are a distraction.

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ARNOLD BLUMBERG. *A Carefully Planned Accident: The Italian War of 1859*. Selinsgrove, N.J.: Susquehanna University Press. 1990. Pp. 238. \$36.50.

The oxymoronic title of Arnold Blumberg's study of the Italian war of 1859 points to that conflict's peculiar characteristic. A plot, unique in modern history, between the leaders of two states to maneuver another into an apparent act of aggression succeeded but under circumstances different from those originally planned and with results unforeseen by the two conspirators. The author's purpose is to analyze the diplomacy of the great powers and of Sardinia from the war's planning stage until its conclusion. In the process he seeks to provide a case study of the differences and consequent tensions between Napoleon III and Camillo Cavour, jointly responsible for the war, linked as allies, but dissimilar in character and circumstances. It is a familiar story presented with impressive thoroughness, clarity, and no surprises.

Blumberg proceeds chronologically, guiding the reader along the road to Plombières and the Franco-Sardinian treaties of January 1859, through the tangled negotiations surrounding the Cowley mission and the Russian congress proposal prior to Austria's fatal ultimatum, across the wartime diplomacy to Villafranca, and finally to the largely abortive settlement at Zurich. Along the way he examines the motives and policies of the statesmen of France, Sardinia, Austria, England, Russia, and Prussia. He also grapples with some difficult problems of interpretation, clarifies certain episodes, and illuminates the role of Alexandre Walewski, Prince Napoleon, Karl von Buol-Schauenstein, and other participants in the proceedings. Concerning the nagging question as to whether Napoleon wavered in his commitment to go to war as tension built in the spring of 1859, Blumberg holds that the emperor never abandoned his intention to fight. This conclusion, persuasively argued, is based on a detailed examination of archival sources, memoirs, and published volumes of diplomatic correspondence.

No new evidence, here or elsewhere, is presented, primarily because little in the way of new primary source material bearing on the diplomacy of the war has surfaced in recent decades. Such a situation has given Blumberg the confidence to publish this book, his doctoral dissertation of 1952, without textual change. Thus, we have the unusual situation of a scholar publishing, after he has established for himself a respected reputation as a diplomatic historian, the work that normally marks the beginning of one's publication record. Blumberg presents an updated bibliography, but there is no reference in his extensive endnotes to any works appearing since 1952. Using them would probably have only marginally affected his presentation. He has exploited in the archives most of the material used by the Istituto Storico Italiano per l'Eta Moderna e Contemporanea for its volumes on the war of 1859. Nevertheless, scholars have provided arguments for and opposition to some of his conclusions since 1952, and a dialogue of some kind with them would have added freshness to his work. A closer look at the impact on imperial foreign policy of domestic developments within France worrisome to Napoleon III would also have contributed to the value of a sharply focused study that can still serve as a useful guide to the diplomacy of a short, limited, but by no means inconsequential, conflict.

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FRANK M. SNOWDEN. *The Fascist Revolution in Tuscany, 1919–1922*. New York: Cambridge University Press. 1989. Pp. xi, 295. \$44.50.

Tuscany's economic and political role in Italy is often overshadowed by that of other regions. Frank M. Snowden's book joins a slim roster of scholarly works in English on the history of Tuscany since national unification. It stresses this region's key role in the rise of fascism. Like Snowden's earlier regional study, *Violence and Great Estates in the South of Italy: Apulia, 1900–1922* ([1986] reviewed in *AHR*, 92 [June 1987]: 703–04), this one concentrates on the transformations occurring in the agricultural economy of the region and on the political significance of the land question. More precisely, it concentrates on the course of agricultural production, land tenure, and the struggle between landlords and tenants in four of Tuscany's eight provinces: Arezzo, Florence, Pisa, and Siena.

This preoccupation with the land is justified by the historical dominance of agriculture and *mezzadria* (a particular form of sharecropping) in the Tuscan economy. Snowden's argument is that the *raison d'être* of Tuscan fascism was the defense of the landlords' authority and rights against threats stemming from the modernization of agriculture and the influx of tenants into militant Socialist labor organi-



zations, dating from the early years of this century. According to Snowden, it was this longstanding class struggle between landlords and tenants that gave Tuscan fascism its peculiar virulence. More than half of the text deals with the land question and fascism in the countryside. The rest considers urban fascism, focusing on the role of business and the lower middle classes. The role of the liberal state in permitting the spread of illegal fascist paramilitary squads is discussed under the revealing heading of "Compromise and Collusion."

Snowden's book has undeniable strengths. It is based on thorough research in police and prefectural archives, the press, and local publications. It features enlightening discussions of production, land tenure, working conditions, and labor contracts. The comparative dimension is sufficiently strong to convey a clear sense of the distinctive features of Tuscany's economy and politics. The book's basic message that the landlord-tenant question was at the heart of Tuscan fascism will be familiar to anyone who is acquainted with the literature on the history of that region. And no one familiar with the literature will be surprised at the author's other conclusions that big business and the liberal state hoped to use fascism for their own ends.

More questionable is Snowden's contention that "the *fasci* to the south of the Apennines were unambiguously the instruments of the landlords" (p. 207) and that radicals in the movement were the dupes of conservative landlords. That fascism for its duration protected the interests of the landlords is clear enough. It is questionable that it did so out of solicitude for the well-being of the landlords. Fascist legislation on *mezzadria* can also be explained by the regime's commitment to a comprehensive policy of ruralization, economic autarchy, and political expansionism. The diaries of Tuscan *squadristi* such as Umberto Banchelli and Mario Piazzesi convey a very different picture of fascism in that region as being motivated primarily by such passions as patriotism, individualism, and antisocialism. Snowden argues that such "petty bourgeois" sentiments served as a mere cover-up to the more sordid business of deciding who would be boss on the land. Perhaps, but convergence of interests rather than subservience may be a better explanation for what happened in Tuscany in 1919–22 and after.

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GUIDO MELIS. *Due modelli di amministrazione tra liberalismo e fascismo: Burocrazie tradizionali e nuovi apparati.* (Ministero per i beni culturali e ambientali pubblicazioni degli archivi di stato, number 10.) Rome: Istituto Poligrafico e Zecca dello Stato. 1988. Pp. 306.

NICOLA TRANFAGLIA. *Labirinto italiano: Il fascismo, l'antifascismo, gli storici.* (Biblioteca di Storia, number 37.)

Perugia: La Nuova Italia. 1989. Pp. xiii, 524. L. 43,000.

Two themes dominate in Nicola Tranfaglia's collection of essays, written between the early 1970s and 1988. He forcefully reasserts the classical interpretation of fascism as a violent and rightist, albeit mass-based, political movement. Over and over, Tranfaglia warns the reader to watch what the fascists did, not what they said. Most of the essays in the first and last sections, "Aspetti del Regime fascista" and "Problemi e note di metodo," are a bitter indictment of Renzo De Felice, Italy's most noted historian of fascism and author of a multivolume biography of Benito Mussolini. In an essay published in 1978, "Interpretazioni del regime," Tranfaglia accuses De Felice of three major errors. The first is to have centered too much attention on the person of Mussolini and, as a result, on personal strategy and psychology rather than on economic interests and social conflict. Tranfaglia believes that much of the distortion arises from De Felice's dependence on fascist archives, although it is precisely De Felice's documentary base that has made his work indispensable to historians of all schools. Tranfaglia also faults De Felice's failure to consult the documentation assembled by contemporary antifascist critics of the regime. Finally, Tranfaglia attacks the whole notion of consensus as misleading, not only because the term is impossible to apply under a dictatorship but also because De Felice ignored the coercion used by the fascist state. De Felice is also accused of stressing ideology over concrete political and economic results. According to Tranfaglia, this error made De Felice exaggerate the importance of revolutionary and left-wing elements in Italian fascism. Distortion of fascism's social base then led De Felice to deny any general concept of fascism and minimize similarities between Nazism and fascism.

Tranfaglia devotes several essays to those radical and socialist historians from Gaetano Salvemini, Carlo Rosselli, and Silvio Trentin to Giorgio Candeloro and Enzo Santarelli who have stressed practice over ideology, repression over consensus, and class interest over personality. An extended study of Rosselli and several well-crafted essays on the "difficult" socialists Rodolfo Morandi and Tristano Codignola and on the republican Fernando Schiavetti accomplish the second main purpose of this book, that is, reassertion of the value and legacy of antifascism and of the Resistance. Tranfaglia links the Marxist and democratic antifascist tradition to the battle against historical revisionists who challenge the values of the Resistance at the same time that they seem to present a more "objective" view of fascism.

Any collection such as this involves a high degree of redundancy. Many arguments against De Felice no longer seem relevant. Although Tranfaglia's main points about the nature of rightist, class-based, and repressive fascism are absolutely valid, such an interpretation does not necessarily support a linkage be-



tween Italian and German varieties of fascism. Nor does it preclude consideration of leftist currents in the fascist movement. In the end, one suspects that this book was published as much for political (in the most honorable sense) as for historiographical reasons.

Guido Melis's fine study is a perfect example of Tranfaglia's admonition to watch what the fascists did and not what they said. Melis traces the transformation of the Italian bureaucratic apparatus from the Giolittian era through World War I and the immediate postwar period to 1929, when the basic outlines of the fascist regime were in place. World War I created opportunities for new bureaucratic models based on technological efficiency and for a new type of bureaucrat who often crossed over from industry. These ministries and agencies broke sharply with traditional Italian administrative practice, but, after the war, calls for bureaucratic reform concealed a basic confusion of aims. Liberals such as Luigi Einaudi used demobilization to demand a substantial dismantling of the bureaucratic apparatus and a return to a simpler model of state. Technocratic rationalizers, associated with prime ministers Francesco Nitti and Ivanoe Bonomi, sought to build on the experience of wartime agencies in their efforts to restructure the administrative apparatus.

Melis notes that the fascists on taking power opted for the liberal simplification model. Alberto De Stefani, an orthodox economic liberal, restored the primacy of the Treasury, the bastion of bureaucratic orthodoxy. During the 1920s the fascists made little attempt to purge or to reorganize the traditional bureaucracy. Politicization, demanded by fascist intransigents such as Roberto Farinacci, was rarely attempted; rather, there was a slow adhesion of traditional bureaucratic elites to the nationalist ideals of generic fascism. In practice, the fascists, while paying lip service to modernization, constructed monumental, old-fashioned government buildings. They failed to change the long midday lunch break by substituting the *orario unico*. They did not reshape the way elites were formed but merely worked for the advancement of selected individuals. Traditional civil servants were successful in subordinating the typically fascist Ministry of Corporations, which Giuseppe Bottai envisaged outside of traditional rules.

If the technical and industrial model made little headway in the traditional bureaucracy, new bureaucratic structures nonetheless emerged in two areas: the massive parastate apparatus and the fascist party bureaucracies. Shortly before World War I, the state began to acquire holdings in the private sector. A first step was taken in 1912 with the Istituto Nazionale delle Assicurazioni, created by Giolitti when the insurance companies were nationalized. The war saw several examples of state and private sector cooperation for munitions production. Then, in 1919, the government acquired a majority position in the railway company that linked Trieste with the former

Austro-Hungarian hinterland. In the early 1920s, fusion between the private sector and the state in which the state acted as a private stockholder was expanded with the creation in 1923 of the Raffineria Olii Minerali di Fiume and the Società Anonima Ansaldo-Cogne and in 1926 and 1927 with state-controlled joint stock companies such as the Azienda Generale Italiana Petroli and companies for natural fertilizers and tobacco.

The architect of fascist parastate apparatus was Alberto Beneduce, who developed a complete capital creation structure outside the control of the Treasury. At the basis of this financial empire were the Consorzio di Credito per le Opere Pubbliche, created in 1919, the Istituto di Credito per le Imprese di Pubblica Utilità of 1924, and the Istituto di Credito Navale of 1928. They were operated as autonomous state agencies to raise capital for public works projects and were run along the lines of private corporations with the state acting in the role of private stockholder. Beneduce applied many of the same techniques in 1934 to the Istituto per la Ricostruzione Industriale, the holding company that took over much of the private sector after the depression-era bank failures.

The second type of bureaucratic structure that found favor under fascism was the parastate agency connected with the ideological or social mission of fascism: for example, the Opera Nazionale Dopolavoro, the Opera Nazionale Balilla, and the bureaucracy connected with the Partito Nazionale Fascista when it became part of the state apparatus.

Melis has turned the dry and technical subject of administrative reform into a pioneering and illuminating study of fascist practice. This economical and well-written study is filled with information on important but lesser-known civil servants and the ministries they guided.

ALEXANDER DE GRAND

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JOHN KOMLOS. *Nutrition and Economic Development in the Eighteenth-Century Habsburg Monarchy: An Anthropometric History*. Princeton: Princeton University Press. 1989. Pp. xvii. 325. \$45.00.

Anthropometric history uses stature, a measure of human nutritional status, as a means of gauging well-being. Its success depends on the quality of the evidence available about stature, the credibility of the idea that nutritional status can be reconstructed on the basis of information about height alone rather than height for weight, and the applicability of anthropometric theory to historical situations. According to this theory as applied by John Komlos, humans have a genetically programmed potential height. The height they actually achieve depends on the quantity and composition of the foods consumed during somatic development, after allowing for the prior claims on nutrients of metabolism, work, and disease.

Komlos adapts the theory to circumstances, the chief of which is that historical sources seldom provide quantitative information about weight or girth. Hence, by definition, populations that are short have a lower nutritional status regardless of whether they are lean, middling, or obese.

In company with other historians and economists, Komlos has exploited sources that record the height and age of people across time. Most of the sources used consist of military recruitment records, which report heights of people nearing completion of somatic development who also meet or exceed a minimum standard. In order to measure average height in the overall part of populations eligible for recruitment, anthropometric historians estimate the heights of people who fail to meet the recruitment standard. Bringing the results of such investigations in the Nordic lands, Britain, and elsewhere together with his own findings, based on original research in Habsburg military archives, Komlos shows that the heights of military recruits born from the 1750s to the 1780s deteriorated. Soldiers who entered service from the 1770s into the 1790s were progressively shorter than their earlier counterparts. Heights remained lower (but did not deteriorate further) among recruits born from the 1780s through the 1830s. Komlos concludes that the well-being of people in Europe in general deteriorated from the 1750s to the 1780s and remained at the low level of the 1780s for several decades thereafter.

This book makes a significant advance in anthropometric history, and many of its features will appeal to historians. Komlos generalizes broadly about European experience on the basis of developments in Austria rather than Britain or France and then uses his reconstruction of Austrian experience as a basis for appraising economic development across Europe in the half-millennium preceding 1800 and especially in the eighteenth century. Komlos designates the key element of this argument as the "Austrian model" of economic development. According to this model the principal change within industrial modernization consisted not of technological innovation but of finding ways to increase industrial output without depriving the populace of the nutrients needed to live. Austria introduced economic development in this form, Komlos argues, through the reforms of Maria Theresa and Joseph II, which promoted manufacturing, safeguarded peasants from extreme hardship, and traded industrial goods produced in the western parts of the monarchy for food grown in the eastern parts. Hence, Austria experienced an industrial revolution in the eighteenth century, meaning that Habsburg policies ended up fostering economic growth without incurring the penalty of a crisis in which population numbers outran nutritional resources. People living in the Habsburg dominions ate enough less to become shorter but not so much less that they died. Furthermore, Komlos theorizes explicitly about the meaning of biological evidence, such

as heights, and economic evidence, such as prices and wages. In his view nutritional evidence provides a means of solving an old problem about whether population growth in the eighteenth century preceded and in some way caused industrial development. Komlos believes that it did, and, moreover, he believes that the distinctive feature of eighteenth-century experience can be described as a shift from a world in which population growth caused immiserization to one in which, in the fashion proposed by Ester Boserup, it caused economic growth.

Equally, historians will be puzzled about whether the evidence can be construed as successfully by means of a nutritional theory as by means of an epidemiologic theory and about how to accommodate many of Komlos's findings with what other evidence suggests about human well-being. They may also be troubled by the bold rather than cautious manner in which Komlos interprets his findings and advances and repeats his views. One of the boldest generalizations is that by the 1780s, 30 to 40 percent of Europeans were chronically malnourished. This means not only that they lacked the calories needed for somatic development to proceed in the ordinary way but also that they did not eat enough to live and work in the way that their predecessors had. Just as bold is the claim that Habsburg economic reforms produced rapid industrial growth. These two claims seem to be at odds with one another. Komlos recognizes this problem and proposes a solution for it. Putative Habsburg industrial development left people who lived closer to food sources better off in terms of the heights they achieved than their counterparts who produced manufactured goods, even though the latter were better paid.

Most historians have assumed that a deterioration in nutritional status, especially one producing such widespread chronic malnutrition, would be reflected in higher death rates. Instead death rates declined, and their decline is usually taken as testimony that well-being improved. Since Komlos does not discuss death rates, incidental mentions aside, it is not clear how he would reconcile their trend with evidence about heights. The boldest claim in this book, however, is that stature alone is an adequate gauge of nutritional status. Nutrition studies indicate that height is attained not only by the volume of calories eaten but also by the contributions of specific foods, of which some (for example, the whole proteins in animal meat or comparatively protein-rich grains, such as rye and barley) promote greater stature and others promote greater weight. It is possible to eat more but grow less.

Taken at face value, Komlos's evidence and theory promote a puzzle of the kind that delights historians. How is the apparent contradiction between evidence about prices, wages, and death rates that suggests improving well-being between 1750 and 1830 to be

reconciled with evidence about heights that suggests deteriorating well-being?

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EVAN BURR BUKEY. *Hitler's Hometown: Linz, Austria, 1908–1945*. Bloomington: Indiana University Press. 1986. Pp. xv, 289. \$27.50.

Austrian history, like that of most other European countries, has suffered from too much concentration on the capital city. An exclusive focus on Vienna can lead to distorted views of the country's alleged poverty and political divisions after the fall of the Habsburg monarchy. Evan Burr Bukey's study of the Upper Austrian capital of Linz serves as a salutary corrective to such distortions.

Bukey's book encompasses both less and more than the title promises. This is decidedly not a book about Adolf Hitler's youthful years in Linz nor even an analysis of his emotional attachment to the city. Hitler's relationship to the city becomes important only during the Anschluss years when he wanted to make Linz one of the five "Führer cities" of the Third Reich. On the other hand, Bukey does not confine his discussion to Linz alone but instead includes the entire province and, when appropriate, the whole country.

Without explicitly saying so, Bukey makes a convincing case for Linz as the second most important city in interwar and wartime Austria. After being a center for German nationalism before World War I, it became a model of democracy in the 1920s, a battlefield during the civil war of February 1934, a focal point during the Anschluss drama in 1938, and a recipient of lavish economic aid from Hitler after Austria's incorporation into the Third Reich.

One of the author's more surprising conclusions is that at least in one of Austria's nine federal states, Upper Austria, democracy worked rather well during the 1920s and early 1930s. In contrast to the federal regime, there was a sense of legitimacy, a commitment to democratic procedures, and a willingness by political opponents to compromise and cooperate. These factors, along with a fairly low unemployment rate and the absence of a university, meant that fascism—both the Heimwehr and the Nazi variety—was contained with relative success.

Despite this comparatively healthy development, Linz and the remainder of Upper Austria undoubtedly supported the Nazi regime until the later stages of World War II. Bukey attributes this popularity to the Nazis' anti-Semitism and to the complete elimination of the region's unemployment within six months of the Anschluss through rapid industrialization.

This study is grounded in all available primary sources and secondary works listed in the author's thirteen-page bibliography. Documents from the Na-

tional Archives, the Berlin Document Center, the archives of the city of Linz and the province of Upper Austria, the Austrian Administrative Archives, as well as twenty-eight newspaper collections attest to the thoroughness of Bukey's research, which began in 1974.

As is virtually inevitable in a book of this scope, there are a few minor factual errors and dubious conclusions. Was the Treaty of St. Germain merely a "seemingly punitive peace" as Bukey maintains (p. 37). The Nazi vote of November 1930 was closer to 110,000 than to the 11,638 cited by the author (p. 94). The Nazis never would have agreed that the dictatorial measures of the federal government of Engelbert Dollfuss were directed more against the Social Democrats than against them (p. 109). And surely the economic boycott of Austria by Nazi Germany was as much responsible for Austria's economic stagnation as the policies of Dollfuss and his successor, Kurt von Schuschnigg (p. 145).

These shortcomings aside, this book is a superb example of regional history serving as an antidote to broad generalizations about national developments. Similarly, it helps explain the economic roots of Nazi popularity and the industrial modernization of Austria.

BRUCE F. PAULEY  
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DUŠAN UHLÍŘ. *Republikánská strana venkovského a maloroľníckého ľudu, 1918–1938: Charakteristika agrárneho hnutia v Československu* [The Republican Party of Farmers and Peasants, 1918–38: The Characterization of the Agrarian Movement in Czechoslovakia]. (Práce oddělení novějších československých dějin, series 1, number 2.) Prague: Ústav československých a světových dějin ČSAV. 1988. Pp. 267.

Although conceived with an intentionally narrow focus, this work is clearly written and well researched, a good addition to our scant store of information about Czech and Slovak political parties. It belongs to a type of overdescriptive case studies. Most of the evidence for the work came from archival collections. As the author of three basic articles on the Agrarian party's policies in the 1920s, Dušan Uhlíř is more than well qualified to offer his contribution.

To the student of modern Czech history, this book, with its heavy emphasis on institutional aspects of party activities, is of real value. The writer points out, quite correctly, that the secondary works in the field have not been available in their treatment of modern Czech politics. He attempts to fill the void. He surveys the organization and typology of the largest pre-1938 Czechoslovak political party, the Republican Party of Farmers and Peasants. The party emerged in the Czech lands in the 1890s and was known popularly as the Agrarian party. It quickly mastered the techniques of mass politics and spoke for Czechoslovakia's

rural population. The party tried consistently to modernize its organization and to broaden its popular base beyond a natural constituency of farmers and peasants.

The author divides his study into two parts. After his examination of the formation, political program, and ideology of the party, he surveys its organizational structures and presents succinct explanations of the effectiveness of the various economic, social, political, and cultural interests that were seeking the representation of the party in the era of the Czechoslovak Republic from 1918 to 1938. The stress on organizational structures permits some points to be made. The political allegiance of cooperative peasant banking and cooperative buying and selling to the party is well illustrated. Encouraged by the party, farmers' marketing cooperatives were formed to perform useful services, provide better storage and grading, as in cooperative grain elevators, and expand branches of agriculture, such as milk and dairy products. In staple crops, the party tried to maintain firm farm prices.

The strength of the author's portrayal lies in descriptions of the variety of affiliated and associated interest bodies, such as sugar beet and potato growers, through which the party had achieved its prominence. The work succeeds in its aim of providing an informative account, although one senses that, appearances notwithstanding, the author could not have extracted from the data all that they contain. The best sections discuss the many links between the political leadership and the economic interests represented by various groups. Unfortunately, the concluding section on the history of the party in the republic is for the most part perfunctory. Moreover, the absence of historical narrative leading the author to overemphasize the organizational aspects of the party activities may well disappoint many historians.

Clearly, more work remains to be done before we are likely to reach any consensus regarding the interpretation of the vital role of political parties in Czechoslovak history. Nevertheless, this case study entitles us to understand more fully the parties' efforts to adapt to the imperatives of modern politics. The notes, which contain valuable bibliographic references and forty-eight pages of appendixes including lists of party leaders and organizations, are of great benefit to the reader. There is no index. Unfortunately, the book's format is ill suited to the importance of its subject. The publisher, the former Institute of the Czechoslovak Academy of Sciences, has produced a shabby, slipshod typewritten volume in two hundred copies "for office use." One can hope that, with the new political developments in the country, future studies of this kind will be published in a way accessible to a scholarly audience.

RADOMIR LUZA  
Tulane University

M. C. KASER and E. A. RADICE, editors. *The Economic History of Eastern Europe, 1919-75*. Volume 1, *Economic Structure and Performance between the Two Wars*. Volume 2, *Interwar Policy, the War, and Reconstruction*. Volume 3, *Institutional Change within a Planned Economy*. New York: Clarendon Press of Oxford University Press. 1985. Pp. xix, 616. \$67.00.

Projected as a five-volume set, the three volumes of this economic history of Eastern Europe under review were written mostly by scholars associated at one time or another with St. Antony's College of Oxford University. The economies of Albania, Bulgaria, Czechoslovakia, Hungary, Poland, Romania, Yugoslavia, and the German Democratic Republic after 1949 are described in a topical manner. Unfortunately, no real justification is made for grouping these countries under the Eastern Europe rubric. What all of them have in common is that they were dominated at least some of the time by the Soviet Union. If geography was not the decisive criterion, then the Baltic countries of Lithuania, Estonia, and Latvia, which had been independent countries in the interwar period and were then forcefully incorporated into the Soviet Union, could also have been included. More properly speaking, these volumes deal with countries in east-central and southeast Europe. They do border on each other, however, and do have some common problems. With the exception of the German Democratic Republic, they have all been members of large empires in the past. For the most part their economies were relatively weak, and their experience with Soviet-type socialism seems not to have been beneficial in the post-World War II era. Along those lines it may be correct to postulate that "the region has enough in common to justify unitary analysis" (vol. 1, p. 2). Moreover, except for East Germany and Czechoslovakia, these countries were characterized by an agrarian economy. In 1920, 65 percent of the region's population of active age were engaged in agriculture. Today 61 percent of the 135 million inhabitants are wageworkers. Nevertheless, there are definite distinctions between well-industrialized countries such as Czechoslovakia and East Germany, on the one hand, and backward countries such as Albania and Bulgaria, on the other.

The cross-country approach applied seems to be somewhat disconcerting. With every subject, even minor ones, one has to read through the record of every country. This is, of course, based on a decision that the editors made. It was more strongly held to in the first two volumes, which deal with the years from 1919 to 1945. Volume 3, composed mostly of chapters written by W. Brus, is certainly more satisfying because these articles are divided in a chronological manner with every country taken up separately.

One of the problems is that the chapters are based on data from individual countries. That is presumably why the predecessor area to the German Democratic Republic is not taken up. It might have been



helpful, nevertheless, to present a short, impressionistic history of the economy of this part of central Germany. The editors may have held too strongly to their decision to start with a certain date and ignore everything before it. It would be easier to understand, for example, Poland's travail in the interwar period, if one realized that the country had been divided into a number of parts since the second half of the eighteenth century. If it were possible to unite all of these countries into one economic union, these populations might enjoy a fairly high standard of living, for, as E. A. Radice, one of the editors and the author of a number of chapters, points out, the area is reasonably well endowed with a number of raw materials necessary for extensive industrial development. Nationalistic attitudes preclude the formation of such a union.

Because these volumes should serve as a standard work, it would have been helpful if demographic elements and the characteristics of the economic geography of the region had been more deeply developed. For example, more could have been said on the soil conditions in the various countries. Reference could also have been given concerning the mountain ranges and the river systems. Especially in respect to the latter, one could have expected a more detailed discussion of the river traffic on the Danube, which goes southeast into the Black Sea, whereas rivers from Czechoslovakia and Poland run in a northerly direction. This is not to say that there is not a sizable amount of relevant material such as tables on basic industrial commodities. Similarly, there is a considerable amount of information on human resources, to which an entire chapter is devoted. For example, there are data on ethnic backgrounds, which are so important for this region, and tables on illiteracy. It would have been helpful, moreover, to have population pyramids to be able to compare the countries of this region with other developed or developing countries. To be sure, there is much relevant material in this category, but these volumes are to be judged on the basis of an ideal and a definitive economic history of the area. In connection with human resources, there is information also on state budgetary expenditures on health and education.

One of the most important chapters in the first volume deals with agriculture, as should be expected. The author, Iván Berend, has written widely on the economic history of the region. It would have been very useful at this point to discuss the quality of the soils. There is, as might be expected, material on land reform in the 1920s. Land reform became a political objective because before World War I there had existed a large number of latifundia in the area. There was a definite land hunger. Smallholder parties were found in most of the countries. It is not clear whether this political pressure should have been dealt with more at this point or in the chapter dealing with the relationship of state to the economy. As Berend

points out, land reform is not necessarily a prerequisite of economic development. At one point Berend makes a rather contradictory statement when he says that "the majority of peasant farms did not have enough to live on but kept above the starvation level" (vol. 1, p. 162). Clearly, there were a large number of dwarf farms that required the owners to derive some of their income from other sources. It is therefore not overly surprising to find that the labor force in agriculture actually rose during the period of 1920 to 1940, whereas in Western Europe it receded somewhat. Thus, it can be said that one-fourth to one-third of the agrarian population in Eastern Europe was actually in surplus during this era. To indicate the backwardness of the area, as late as 1938 the three-field system was still being used in some parts.

The chapter on industry, written by Alice Teichova, is also a noteworthy contribution. We find that Czechoslovakia was, as is well known, the most industrialized country in the interwar period. With 15 percent of the population of Eastern Europe, it produced 40 percent of the industrial output of the region. The increasing rural population in the region could not be conveniently absorbed into the industrial sector as was the case in Western Europe in the nineteenth century. The experience of Eastern Europe does not confirm the hypothesis of Alexander Gerschenkron that late industrializers benefit from the accomplishments of the pioneers. In all of the East European countries, the majority of the industrial workers were employed in a small number of large enterprises. Also there was a substantial cartelization, especially in Poland and Czechoslovakia.

The chapter on foreign trade by Z. Drabek is replete with extensive tables on imports and exports. The amount of detail is probably greater than required for a survey of the topic. Yet the discussion of tariffs to be found in the chapter on the role of the state could more properly have been included in the chapter on foreign trade. Nevertheless, it is interesting to note that the newly created countries took over the prewar tariffs of the empires that they had left. This was especially the case for the successor countries of the Austro-Hungarian monarchy. By 1930 the aim in all of the pertinent countries was to exclude free competition in the shaping of agricultural prices. In many of these countries, the state owned a significant amount of industry and transportation. There was also some state planning, especially in Poland.

Milan Hauner presents an interesting chapter on military budgets and the armament industry. The Czechoslovak industry was of major importance militarily. The Škoda works, which employed thirty-two thousand workers in 1937, was the most outstanding firm. It eventually was incorporated into the Hermann Göring Werke. If there is a criticism about this chapter, it is that it is too detailed for an economic history of the entire region. Moreover, the currencies



that are referred to are in national currency demarcation. It would have been more useful to translate them into some world currency such as the American dollar or the British pound sterling.

All in all, this set of books is extremely useful. Any student of east-central and southeast Europe cannot pass up these volumes.

HERMAN FREUDENBERGER  
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ALBERT J. SCHMIDT. *The Architecture and Planning of Classical Moscow: A Cultural History*. (Memoirs Series, number 181.) Philadelphia: American Philosophical Society. 1989. Pp. xiii, 218. \$38.00.

In the view of Albert J. Schmidt, "classical Moscow became an exemplar for the architectural tradition of early nineteenth-century Europe" (p. 8) and, as such, deserves to be ranked with St. Petersburg, Berlin, Munich, and Vienna. Schmidt's work underscores the importance of the initiatives taken by Catherine II and Alexander I in reshaping the city. Like Peter the Great, Catherine disliked Moscow. Particularly after plague killed about one-quarter of its population in 1771, she worried that the persistent threats of pestilence, fire, and civil upheaval in this "seat of sloth" (p. 41) might undermine her realm. Reluctantly, she decided to redesign the city along classical lines. Schmidt describes the ensuing plans and projects in considerable detail, focusing on those architects who "took their commissions from Moscow's grandees, whose good life Tolstoi portrayed in *War and Peace*" (p. 8).

Most notable were Vasilii Bazhenov and Matvei Kazakov, who refashioned the Kremlin along classical lines and set into motion plans for fundamental changes in the rest of the city. Bazhenov was the original designer, for example, of Pashkov House (1784–86), well known to scholars today as part of the Lenin Library. Although many of their plans and projects did not materialize, or were delayed until after the great fire of 1812, Catherine's Moscow increasingly became a city of ocher and amber classical structures: "In looks, if not in spirit, it was, indeed, becoming a part of Europe" (p. 99).

Fire had long plagued Russia's wooden cities, but it did provide opportunity for experimentation in rebuilding. Tver, reduced to ashes in a single day in 1763, became one laboratory for planning schemes. The Moscow fire of 1812, set perhaps by Napoleon or by the Russians themselves, spared the Kremlin but destroyed much of the rest of the city, including many of the classical structures built under Catherine. Alexander I was committed to an empire of classical cities, however, and rebuilding Moscow, the scene of Napoleon's greatest defeat, became "a compulsion" (p. 128) for him. The Garden Ring began to evolve at this time. Many of the changes, particularly those in Red Square, Theatre Square, and the area

around the Manezh, were inspired by Osip Bove, whose efforts have been insufficiently recognized, Schmidt believes. More than anyone else, Bove "determined the character of central Moscow from that day until our own" (p. 146).

Classicism waned in the 1830s, only to reappear in planning and architecture in the early twentieth century (Spaso House, the current home of the American ambassador, is one example) and again under Stalin. Schmidt outlines the controversies over the extent to which foreign versus indigenous factors have shaped Russian classicism. Soviet authorities generally have held classicism in high regard but, nonetheless, except for Leningrad, have destroyed many of its best examples. In the 1960s the modernistic Kalinin Prospekt "knifed through the old Arbat, thereby destroying the heart of classical Moscow" (p. 201).

Schmidt has included seventy-seven illustrations, many of them his own photographs. Particularly interesting are the old prints and photographs taken from Moscow's Donskoi Archive. Schmidt's descriptions of various projects are often detailed and sometimes quite technical, and, although there are good maps, some readers may have difficulty with the many references to Moscow streets, neighborhoods, and districts. This attractively produced book will therefore appeal primarily to those who are well versed in architectural history and those who know Moscow well.

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MARCUS C. LEVITT. *Russian Literary Politics and the Pushkin Celebration of 1880*. (Studies of the Harriman Institute.) Ithaca: Cornell University Press. 1989. Pp. x, 233. \$27.50.

The Pushkin Celebration of 1880 lasted three days, June 6–8, and included public speeches, banquets, a church service, literary and musical presentations, and an elaborate unveiling ceremony of a monument to Alexander Pushkin. The list of attendees included such leading lights of Russian culture as Ivan Turgenev and Fedor Dostoevsky. Yet the celebration is remembered chiefly as the occasion for Dostoevsky's speech on Pushkin as the prophet of Russia's universal mission.

Marcus C. Levitt's study situates the "Pushkin Days" in the larger context of literary criticism, Russian national and cultural aspirations, and political developments and shows that Pushkin, a writer comparable to Dante, Shakespeare, and Goethe around whom Russians of different cultural, political, and social views could rally, represented Russia's claim to cultural parity with the West. The author argues that the "Pushkin Days" were conceived as a "liberal event" even though they turned out quite differently.

The argument is made in five chapters. Chapter 1, "The Debate is Formulated: The Question of a Monument to Pushkin, 1837–1866," describes the origins of the celebration in the idea of a monument to Pushkin that was made the day after his death, Tsar Nicholas I's prohibition of such a monument, attacks on Pushkin by radicals and reactionaries, and the beginnings of a subscription campaign for a monument in 1861. Chapter 2, "Those Who Kept the Light Burning: Working Towards a Monument, 1869–1880," describes how alumni of Pushkin's Lycee made him into a cult figure and revived the faltering campaign. Chapter 3, "The Celebration That Organized Itself," covers the events that turned the unveiling of the monument into a celebration of Pushkin, who personified the organizers' hopes for free speech, personal autonomy, and a greater voice in public life. A convergence of forces contributed to their success. The celebration occurred during a "thaw" in relations between government and society, the "political spring" of Loris-Melikov, when both sides desired some sort of rapprochement. Public interest was aroused by intensive press coverage that treated the celebration as a vindication of *glasnost*' (the term was commonly used in the reform era), last minute scandals, the hope for reform, and expectations of a showdown between Westernizers and Slavophiles.

Chapter 4, "Turgenev's Last Stand," discusses Turgenev's central role as the standard-bearer of Russian liberalism and explains the lukewarm reception of his eagerly awaited speech. Delivered in a "weak and high-pitched voice . . . which broke into falsetto during moments of agitation" (p. 110), the speech neither satisfied the public's desire for a forceful statement of Pushkin's universal importance nor provided a focal point for the celebration itself. Turgenev's inner reservations on the feasibility of reform and his refusal to idealize Russian reality blunted the speech's impact and underscored liberalism's weakness and vulnerability. Chapter 5, "Dostoevsky 'Hi-Jacks' the Celebration," accounts for the ecstasy that greeted Dostoevsky's speech by explicating its religious subtext, which struck deep cultural chords, notes the impact that Dostoevsky's passionate belief in his own message had on the audience, and points to Dostoevsky's appropriation of the word "liberal" as an indication of its ambiguity in the Russian context. Using phrases such as "positive moral beauty" (p. 129), Dostoevsky fused messianism and aestheticism; advocated politics on a new basis, a community united in Christian love; and praised Pushkin as a prophet sent by God to lead the straying intelligentsia back to its true Christian path, thereby saving both Russia and Europe.

The concluding essay, "Aftermath and Legacy: Pushkin, 1880–1987," shows that although hopes for reform were squelched by the assassination of Tsar Alexander II, the celebration established Pushkin's mythic role as the redeemer of Russian civilization.

Subsequent celebrations of Pushkin occurred under government auspices in 1899, 1921, and 1937; he was important to symbolists, acmeists, and futurists. Soviet Pushkinologists are still discussing his legacy.

This truly excellent book restores the Pushkin Celebration to its historical importance, documents the interplay of literature and politics before, during, and after the event, and raises the larger issue of the viability of a liberal position in Russia. Noting the ambiguity of the very word "liberal" in the Russian context, Levitt considers the success of the celebration, which surprised even its organizers, to be a testimony to its broad appeal and deems Tolstoy's refusal of Turgenev's invitation to participate, Mikhailovsky's radical critique of liberalism, and Dostoevsky's ability to "hi-jack" the celebration evidence of its vulnerability and weakness. At the same time, he finds a kind of ultraliberalism in Dostoevsky's speech. Yet, although Dostoevsky seems to have been caught up in the general euphoria of "political spring" when he wrote the speech and expected some sort of apocalyptic solution to Russia's problems, he never explicitly renounced his early "reactionary" views. Whether he would have done so had he lived (he died just before the assassination of Tsar Alexander II) remains an open question.

The book is written very much with contemporary events in mind. Guardedly optimistic on the future of *glasnost*' and *perestroika*, Levitt maintains that liberalism requires an educated middle class, which now exists in the Soviet Union. If his prediction of the ultimate liberalization of Russian society comes true, however, it may be because of yet another factor, new for Russian intellectuals—the recognition of the link between a market economy and personal freedom—the same economic freedom that Russian radicals decried and most Russian liberals ignored.

BERNICE GLATZER ROSENTHAL  
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ANDREW M. VERNER. *The Crisis of Russian Autocracy: Nicholas II and the 1905 Revolution*. (Studies of the Harriman Institute.) Princeton: Princeton University Press. 1990. Pp. x, 372. \$35.00.

As Andrew M. Verner notes in his introduction to this thoughtful and carefully researched monograph, the personal role of Tsar Nicholas II in the extended crisis of the autocratic system of late imperial Russia has been taken for granted or largely ignored by serious scholars. Considerable attention has been paid to the tsar's various opponents and to his bureaucratic servitors, but the autocrat himself is generally dismissed as a weak and almost inconsequential figure. Those who suggest that the system could have been reformed tend to play down his potential impact or to argue that his impotence only weakened resistance to reform. Those less optimistic about the system's potential for change usually rest content with

some version of Leon Trotsky's famous argument that a debilitated autocracy got the enfeebled autocrat it deserved.

Verner stands in the pessimist group, but he is rightly dissatisfied with deterministic explanations. Nicholas II, he presumes, played a key role by virtue of his position, whether he was weak or strong, passive or active. Integrating a sober if, perhaps, too unimaginative appraisal of the tsar's personal psychology with a subtle, sophisticated, and quite penetrating investigation of the vicissitudes of autocratic ideology in a time of crisis and rapid change, Verner offers a detailed, cogent, and persuasive account of high state politics during the pivotal events surrounding the Russian revolution of 1905.

According to Verner, Nicholas II was psychologically incapable of synthesizing and internalizing a coherent conception of his own role and of autocracy in general. This proved fatal to his cause because he took the throne at a time of profound ideological ferment among his supporters. From Nicholas II's point of view, the struggle between autocracy and democracy was comprehended through the prism of a conflict among competing conceptions of autocracy. Confronted by changing and antithetical alternatives, the tsar simply declined to choose. He became the captive of events because he could neither take on the role of all-powerful tsar-father to which he was emotionally attracted nor adopt to bureaucratic or constitutional versions of his role.

In October 1905, Nicholas granted a semi-constitution with great reluctance. But he interpreted this act less as a concession to constitutionalism and popular sovereignty than as a way to continue avoiding a commitment to the bureaucratic reformism of high government officials, especially that of Count Sergei Witte. Granting representation, he seemed to believe, was a way of reestablishing traditional links between tsar and people that both bureaucrats and democrats sought to disrupt. With the bureaucracy now severely undermined, however, the tsar was reluctant to accept societal participation in government and ultimately was unwilling to assert his own will. Even had he done so, it is doubtful that he could have succeeded. Because the tsar could never define his own function, his resolution of the real issue remained equivocal and inadequate. Hence, Verner contends, "there was no realistically conceivable way after 1906 that autocracy could have reformed and adapted itself enough to the demands of society in order to avert a revolutionary outcome" (p. 6).

Verner brings a keen intelligence to his subject, and I found myself in general sympathy with his argument. But, in the end, the book was slightly disappointing. Despite the author's diligent research in archival sources, I did not conclude the volume with a sense that much that is new and dramatic had been revealed. The book is most successful in its fruitful approach to the problem of autocratic ideology, but its portrait of the autocrat leaves something to be

desired. Readers will gain considerable insight into previously underexplored political and ideological dimensions of the Russian crisis, but Nicholas II remains a shadowy figure. This is probably not Verner's fault. It is difficult, after all, even to imagine a truly vivid and gripping study of a leader who "comes across as a passive object enveloped by formless chaos, carried along by an unfathomable fate to which he has no choice but to submit" (p. 242).

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LYNN GARAFOLA. *Diaghilev's Ballets Russes*. New York: Oxford University Press. 1989. Pp. xviii, 524. \$29.95.

Vaslav Fomich Nijinsky, Sergei Pavlovich Diaghilev, the Ballets Russes, *Le Sacré du printemps*, the Paris riot—all are a romantic memory of a vanished age. Lynn Garafola has re-created that time in a comprehensive and original manner. Her book is not a straight chronological history. Rather, she has divided it into three sections. The first traces the artistic evolution of ballet from St. Petersburg in the 1890s to prewar Paris where Diaghilev founded the Ballets Russes in 1911 and concludes in 1929 when Diaghilev died, as did his unique institution. The next segment, entitled "Enterprise," is devoted to the various means that Diaghilev used to cajole money from his herd of admirers in Moscow, St. Petersburg, Paris, and London. Great art has never come cheaply, and, during the twenty years of the Ballets Russes, Diaghilev was often forced to beg, borrow, and (almost) steal in order to stay afloat. The final section, "Audience," is in truth a thoroughgoing sociocultural history of the extraordinary figures of the European elite—financial, aristocratic, and artistic—who admired and lent support to the Ballets Russes.

Garafola's research is admirable. There is an extremely useful "selected" bibliography, one hundred pages of many fascinating endnotes, and a helpful chronological list of all of Diaghilev's ballet and opera productions in Paris, London, and Monte Carlo between 1905 and 1929.

This is a dense book. Every important European painter, writer, aristocrat, playwright, banker, society mogul, hanger-on, dancer, critic, impresario, and grand duke is mentioned—everybody who was anybody (and some who were not) of the age. We are bombarded with names, places, and dates, all of which touched on the peripatetic wanderings of Diaghilev and his troupe.

At the center of all this is Diaghilev. Although he is mentioned on every page, we do not know him, nor for that matter his most renowned prodigy Nijinsky. The author suggests the impossibility of getting to the heart of the man: "Here, as on many occasions, one would like to probe Diaghilev's motives, but the man behind the public figure remains inscrutable"

(p. 313). Although Nijinsky's balletic and choreographical originality and genius are well described (*Les Jeux, L'Après-midi d'un faune, Le Sacré du printemps*), he, too, remains a mystery, save for cursory mentions of his homosexual relationship with Diaghilev, their breakup, his madness, and a desultory comment from a member of the Bloomsbury set who found Nijinsky inarticulate.

Despite the avalanche of detail, which at times can be numbing, the book does give the reader a powerful vision not only of the greatness (and decline) of the Ballets Russes but of the complex and sometimes sordid world of artists and lovers of art in prerevolutionary Russia and in France and England in the "good old days" before World War I and, at least for the author, the deprived and somewhat depraved era of the 1920s. World War I caused a sea change in modern history, and art, the perception of art, and the audience for art were not the least affected.

This is not an easy book to read, but the effort is rewarding. One longs to be in the theater, to share in the wonder of the original *Sacré*, *Parade*, or *Le Boeuf sur le toit*, to gaze at Léon Bakst's scenery or the curtain painted by Picasso, and to gawk at opening night Parisian audiences *à la recherche* of figures who were literally a part of Marcel Proust's world. One cannot, of course, for it is impossible to re-create an artistic event out of time and place. But Garafola has made us appreciate that age and the glory of its greatest balletic impresario. She concludes that Diaghilev's genius was "to marshall talent in the creation of an art that touched the consciousness of an era—who today can claim even to aspire to that?" (p. 378)

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JOSEPH T. FUHRMANN. *Rasputin: A Life*. New York: Praeger. 1990. Pp. x, 276. \$24.95.

Grigorii Rasputin, the Siberian peasant charlatan whose ready access to the inner circle of Nicholas II and his wife Alexandra appalled responsible Russians at the beginning of this century, has been a favorite subject for writers searching for scandal to darken the twilight of imperial Russia. Their accounts have portrayed Rasputin as a sinner, a saint, a devil, or a deity dedicated (in the opinion of some) to Russia's salvation or responsible (in the view of others) for the outbreak of revolution and the fall of the Romanovs in 1917. Over the years, we have been introduced to Rasputin "the Mad Monk," Rasputin "the Holy Devil," and Rasputin "Neither Devil Nor Saint" to quote from some of the most striking subtitles that have appeared. We have had a book that promised to show us "the Real Rasputin" (by his daughter), another that promised to reveal "the Man Behind the Myth" (also by his daughter), and a number of others, the titles of which promised less but supplied no more.

Every piece of gossip, every bit of scandal, and every hint of perversion connected with Rasputin has been chewed over by writers anxious to highlight his influence peddling or recount his sexual escapades among St. Petersburg's females. The consistently low quality of such work makes a reader wary of anything written on the topic. One comes to any new volume about Rasputin—especially one that its publisher advertises as "an unforgettable journey into the life and times of Grigory Rasputin"—with a powerful sense of *déjà vu*. Almost without meaning to, one asks: Do we really need to read all this again?

In the case of Joseph T. Fuhrmann's biography of Rasputin, the answer is a surprising and emphatic "yes" that underlines again the fact that good popular history is best written by professional historians who have the training, willingness, and patience to analyze the source materials at hand. Fuhrmann resists the temptation to highlight scandal, shape gossip into the fabric of history, and emphasize sexual intrigues and sexual perversity at the expense of careful analysis and humane understanding. His book carefully chronicles its subject's influence on Russian affairs during the decade before the revolution of 1917 and makes a serious effort to evaluate the impact of Rasputin on the course of Russia's history.

To write about Rasputin in this fashion, Fuhrmann has had to sift through an immense quantity of sources that are heavily laced with rumor and gossip, test them against each other, and extract the grains of truth from the proverbial mountains of sand. This is precisely where earlier studies of Rasputin have failed so miserably, and it is to Fuhrmann's great credit that he has produced a solid historical biography, not another recounting of Rasputin's escapades through the brothels and boudoirs of St. Petersburg. Fuhrmann's examination of Rasputin's religious views is easily the best to appear anywhere. And his study of precisely how, why, and to what extent Rasputin gained a prominent place in the political intrigues of St. Petersburg during World War I is well and carefully done.

As a chronicle of the weaknesses of the inept men among whom Russia's last empress and emperor felt most at ease and with whom they surrounded themselves, this study serves as a much-needed corrective to the sentimental vision of Russia's last rulers that continues to endure despite the overwhelming evidence that has accumulated against it over the past seventy years. If Fuhrmann's volume has a failing, it is that the tragedy of World War I and the gathering clouds of revolution remain slightly out of focus and fade too far into the background. But such criticism is not meant to detract from the author's accomplishment, which is considerable. The book is a portrait of a complex and, at times, much-confused man who allowed forces he could not control to draw him too deeply into political undertakings that he did not understand. That he did so to the detriment of his imperial benefactors and of Russia was as much their



fault as his, a conclusion that Fuhrmann's careful research makes abundantly clear.

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DIANE P. KOENKER and WILLIAM G. ROSENBERG. *Strikes and Revolution in Russia, 1917*. Princeton: Princeton University Press. 1989. Pp. xix, 393. \$39.50.

Advocates of "new thinking" in the Soviet historical profession now concede that in 1917 many Russian workers had "a naive belief in a social utopia, an impending realm of freedom, equality and fraternity" (V. P. Buldakov, "1917 god: bol'shinstvo naroda i revoliutsionnoe soznanie," *Voprosy istorii KPSS* [1989], no. 6, p. 54). But Diane P. Koenker and William G. Rosenberg see Russian proletarians as rapidly acquiring revolutionary class consciousness, notably through the massive strikes they launched against exploitative capitalist entrepreneurs backed by the Provisional Government. In this way the October overturn appears as an authentic expression of the popular mood. Western "revisionist" sovietologists sometimes sound curiously like Brezhnevite conservatives.

Whatever one thinks of the authors' interpretation, they deserve credit for their industry, the fruit of rare scholarly cooperation. They have assembled a corpus of statistical data on 1,019 strikes during the eight months between March and October 1917, drawn largely from the contemporary press and Soviet documentary collections. Commendably, they tell us in detail how this information was computerized, what precautions they took to minimize bias, and what analytical procedures they adopted. Their conclusions modify earlier, more impressionistic views in several important particulars. For instance, it emerges that industrial action was "clustered" in three distinct periods, each of greater intensity; that the metalworkers' relative preponderance in strike activity declined after July 1917 but that leather workers were even more "strike-prone"; and that semiskilled men played a key role. Other findings support conventional assumptions, for example, that strike waves spread outward from the capital cities and from heavy industry to the service sector.

Despite the authors' acknowledged statistical expertise, qualms persist about their methodology. It is an exaggeration to say that, in 1912-14, "the average annual proportion of strikers approached three-quarters of the factory labour force" (p. 25), for the data on the size of the work force and the annual number of strikers (pp. 30, 58) indicate that the true figure was closer to one-third. Another puzzle is the proportion of strikers who "challenged managerial authority" during the spring of 1917: this is given differently in tables 8.3 and 9.4 (and omitted from table 5.4). In general the number of workdays lost would have been a more accurate measure than the

number of strikers. The authors' explanation of why they disregarded these data (p. 338) is inadequate, because the differential could surely have been calculated from such partial information as was available (for 403 strikes).

The perspective is also slanted. It is arbitrary and misleading to single out strikes from other facets of the industrial scene (for example, plant seizures, absenteeism). We hear nothing about the ethnic composition of workers in minority regions, which often made for social conflict, or about industrial labor's growing disillusionment with Russia's participation in World War I. If indeed only 2 out of 461 strike demands mentioned this subject (p. 350), it would seem that these *cahiers de doléance* are an unreliable guide to emotions on the shop floor.

Most disturbing, however, is the authors' total indifference to the economic impact of strikes. Russia's industrial and financial system was already suffering severe dislocation. What did labor troubles add to declining output and productivity or to galloping inflation? Were employers' complaints on this score well founded? The wretched "bourgeoisie" seem to do nothing right here. Instead of objective analysis, we get only the familiar negative stereotypes (for example, pp. 242-43); instead of social reality, we hear about the mutual "perceptions" entertained by representatives of capital and labor. It is clear that, by the fall of 1917, striking for higher pay had become counterproductive. But for Koenker and Rosenberg, "rising prices and threatening food shortages increased the necessity for higher wages and benefits just as they were becoming significantly more difficult to obtain" (p. 263). They seem to assume that state regulation of the market could have cured industrial conflict and other economic ills: at least the merits of such policies are nowhere called into question.

Last, this "depoliticized" history does less than justice to the role of all potential leadership groups, from the left-wing parties to trade unions and factory committees. Bolsheviks are scarcely to be seen anywhere. This myopia leads to a misunderstanding of the term "mobilization," identified here with accrued militancy rather than with the purposive coordination of efforts to achieve an ulterior goal. The continual tension between rival cadre organizations certainly did not stop them from trying to "organize, direct or even mobilize activist workers" (p. 104), and it is naive to assume that they merely reflected the masses' own elemental aspirations. Would Koenker and Rosenberg say the same of recent tumultuous events in Eastern Europe? In sum, this study adds to our knowledge of the Russian revolution but also perpetuates the myths that have long enshrouded it.

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ZIVA GALILI. *The Menshevik Leaders in the Russian Revolution: Social Realities and Political Strategies*. (Studies



of the Harriman Institute.) Princeton: Princeton University Press. 1989. Pp. xviii, 452. \$45.00.

Ziva Galili's long-awaited study of the Mensheviks during 1917 is a tightly focused, well-written, and persuasively argued history of the Menshevik Defensist leaders. The author concentrates most of her attention on the first few months of the period following the February Revolution, when the moderate Menshevik leaders dominated both their party and the soviets and, in effect, wielded virtual governmental power. The July-October period, when the Defensists lost their influence, is summarized in one (the last) chapter. This definition of the topic is broader than it seems, for Galili actually examines the interrelationship among three groups: the moderate Menshevik leaders; the entrepreneurs, who constituted the backbone of the liberal bourgeoisie on whom Menshevik plans depended; and the labor movement, which would determine the success or failure of the Menshevik-bourgeois alliance. The author deftly interweaves the story of these three groups and firmly establishes the wisdom of so defining (and delimiting) her areas of analysis.

Rather than attempting to revise the basic historiography of the revolutions of 1917, the author aims at deepening our understanding of certain important aspects of that fateful year, namely, the successes and failures of the Menshevik Defensist leadership. Galili agrees with the standard interpretation that the Menshevik leaders were too dogmatic about the stages of revolution: they believed that a prolonged bourgeois phase was unavoidable in Russia and that socialism was for the future. Furthermore, whatever other differences they had, all shades of Menshevism abhorred the concept of socialist participation in a liberal government. Thus, ideology played a great role in the Menshevik leaders' activities. Yet the author insists that during the first months of the revolution the Menshevik leaders displayed considerable flexibility and creativity in their approach to important questions. Their mediation helped to create, at least at first, a surprisingly good atmosphere between workers and entrepreneurs. When this arrangement began to break down in April and May, the Menshevik Defensists again transcended ideological restraints by entering a coalitional government with liberals. When workers then predictably blamed the Mensheviks and Socialist Revolutionaries (SRs), who sponsored and entered the coalition, for its shortcomings, however, the Menshevik leaders resorted to ideology in refusing to countenance an all-socialist government and wound up watching their following, influence, and, indeed, relevance melt away.

The critical edge of Galili's analysis is one of the impressive aspects of the book. She believes that the Mensheviks' legislative record in the Provisional Government was meager and, as noted, that they never escaped their schematic view of history. Still, the

author might have explained more fully why the Menshevik leaders at first displayed flexibility, whereas later they marched to a more dogmatic tune. A somewhat more serious problem regards the Right Menshevik-Right SR bloc, which was of much greater duration (1917 and 1918) than the Menshevik-liberal alliance. Yet the author says much less about the SRs than about the bourgeoisie. Granted the importance of the liberal alliance for Menshevik Defensist plans for the post-February era, the Menshevik-SR bloc was also very significant, and Galili should have discussed it in greater detail.

Galili implicitly warns against mechanical interpretations of history. The greatest achievement of this book, which should become standard reading about 1917, is that it shows how specific historical events occur in concrete circumstances. Although Galili's study may not alter basic interpretations, it helps us to understand the crucial role of the Menshevik Defensists in the 1917 revolution much more clearly than we did before.

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ORLANDO FIGES. *Peasant Russia, Civil War: The Volga Countryside in Revolution, 1917-1921*. New York: Clarendon Press of Oxford University Press. 1989. Pp. xvii, 401. \$68.00.

Although the Russian Civil War is generally recognized as a powerful and formative event in Soviet history, it remains, outside the USSR, one of the most neglected and least studied periods in the Soviet experience. For this reason alone, historians should welcome Orlando Figes's detailed, extensively researched book on the Volga countryside, 1917-21.

Rejecting crude Soviet formulations from the pre-1985 period, as well as facile Western interpretations that impose control over the villages by Red commissars and outsiders, Figes focuses on the "social forces behind the consolidation of the Bolshevik dictatorship in the countryside" (p. 9) and on what he calls the "missing counterrevolution" (p. 3). Figes seeks to explain how the Bolsheviks won the Civil War, despite the countryside's widespread resentment of the policies of War Communism, and why the Volga-area peasantry for the most part remained, in his view, loyal to the Bolsheviks, whose policies were often alien to the peasantry's interests.

The author explores events in Samara, Saratov, Simbirsk, the Penza provinces, and the Autonomous Republic of Volga Germans and occasionally considers developments in Tambov, Voronezh, and Kazan as well as the regions around Ural'sk and Ufa. Most of the data concerns Samara and Saratov provinces, especially Samara. Figes's account of the Volga peasantry in 1917 is largely derivative, but his treatment of the first six months of peasant rule after the October Revolution is highly original, and he offers a

convincing explanation of how social revolution was carried out and how the transformations in the countryside helped consolidate the Bolshevik dictatorship. Left to its own devices, the peasantry reformed the rural areas through its own autonomous organizations (especially the revitalized commune) and according to traditional notions of social justice. This popular revolution of 1917–18 destroyed the old regime and undermined at a critical moment the anti-Bolshevik forces that rallied behind the Constituent Assembly. Figes dismisses what Soviet historians label a “kulak counterrevolution” in mid-1918 and, in analyzing the peasantry’s relationship with the Komuch, shows how peasant fears of the Whites prevented them from emerging “as the main social force of the anti-Bolshevik movement” (p. 154).

Drawing on heretofore untapped archival material, Figes is at his best in probing the anatomy of the village and volost soviet elections of 1919, held in the wake of the lamentable experiment in setting up committees of the village poor. Also noteworthy is his description of the rural economy under War Communism, even if he steers clear of the debate on the policy’s origins. The author has fresh things to say about Bolshevik food procurement policies, peasant agriculture, rural industry, collective and state farms, and relations between the peasantry and the Red Army. What emerges is a scenario in which the social pressures of the Civil War, which destroyed village democracy and recast economic activity, gave rise to a rural arm of the new party-state apparatus with a massive standing army and a swollen bureaucracy under party domination. The peasantry’s drive to reestablish peasant rule locally against the state resulted in an explosion of small peasant wars, the topic of Figes’s final chapter, in which “the Bolshevik government surrendered before its own peasantry” (p. 321).

A short review cannot do justice to the richness of Figes’s discussion or to the many points worthy of comment found in this excellent study. Yet the book is not successful in placing peasant Russia in broader context. The author’s field of vision induces him to simplify developments in the Volga towns and, at times, to both dismiss the powerful reach of Moscow and exaggerate it. As a result, the texture of local politics is somewhat coarse, and the role of political parties in the villages, especially that of the Left Socialist Revolutionaries and the Revolutionary Communists who superseded them, is underestimated. Clearer assessment of the leveling effects of land transfers and deeper treatment of changing peasant attitudes toward the commune and support for private farmsteads (*khutora*) would also have been welcome. Figes is too hesitant in ascribing blame for the horrific famine of 1921–22. His terse conclusion suggests how the Civil War created a pattern in peasant-state relations that was resolved tragically later but does not do justice to the meaty evidence that the author provides his readers.

Finally, the reader might also feel uncomfortable with an ambiguous image. Figes remarks that the “Bolshevik government surrendered before its own peasantry,” yet he sees the government’s defeat of the peasant uprisings as unavoidable. The Volga peasants, to whom Figes gives muted voice, were not counterrevolutionary, but neither, for that matter, were they loyal to the Bolsheviks.

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CHRIS WARD. *Russia’s Cotton Workers and the New Economic Policy: Shop-floor Culture and State Policy, 1921–1929*. (Soviet and East European Studies, number 69.) New York: Cambridge University Press. 1990. Pp. xvi, 304. \$44.50.

The history of the Soviet economy has been dominated by the paradigms of industrialization and central planning. Industrialization, as the argument goes, created a cadre of factory proletarians whose political support was tapped for the socialist revolutions of Lenin’s time and Stalin’s time, and central planning permitted the application of technology and scientific management for maximum efficiency. The paradigmatic factory proletarian was opposed to a paradigmatic peasant-worker. Tensions in society and the workplace could be attributed to the clash between competing sets of urban and rural values. Chris Ward’s fine new book demonstrates how unhelpful these paradigms really are in interpreting the development of industrial policy and practice in the 1920s and, in so doing, makes a major contribution to labor history and to the history of the USSR.

Ward’s study deals with the industry whose success was most important in bringing the fruits of revolution to Russia’s majority population. Cheap cotton was the linchpin of the peasant-worker alliance. Ward looks at the industry from the perspective of shop-floor culture, defined not so much in terms of social patterns and everyday life as in terms of what others call the labor process: the technological make-up of industry, the organization of work, and the reproduction—training—of workers. The keys to this culture and to the industry were small cohesive work units, rather than atomized individual machine-tenders, and technological variability. Implementation of uniform central policy on norms, wages, work rules, and productivity was virtually impossible because of the utter lack of technological standardization in the mills of Leningrad and the Moscow industrial region. The “proletarians” were also far from standard. Ward finds the difference between local peasant-workers and faraway peasant-workers as important as the characteristic of landholding itself. Differences in gender, age, and family position also added to the complexity of shop-floor relations. The world of work was both highly complex and intensely localized.

This was the world that outside agencies—planners, managers, trade unions, and the Communist party—had to control to produce the goods the country needed, but control was impossible. Ward traces the organization of the textile industry and shows how endless reorganizations never achieved the goal of higher production at lower cost. The application of certain technologies, such as the ring frame, that in England led to greater managerial control, failed to do so in Russia and instead reinforced shop loyalties and particularism. The organization of work by teams, traditional gender roles, kinship networks, technological idiosyncracies, and uneven literacy reproduced and transmitted a traditional culture of work that generated passive resistance to the flood of decrees and directives from the rationalizers in the center.

Ward discusses these structural limits in the context of the recovery and development of the industrial economy after 1921 and shows how successive attempts at reform—the Scientific Organization of Work movement (NOT), speedups, production conferences, third shifts, and more speedups—reacted imperfectly to crises of unemployment, shortages of raw material, international crises, and the reluctance of peasants to market their grain.

This is a richly documented and important work. Ward used no archives for the study (begun before *glasnost*) but made extensive use of local newspapers and trade union publications. He provides a model of how these sources can illuminate local life and complex problems. This discussion is at times dense and occasionally presumes an advanced knowledge of the textile production process and English economic history. The mass of specific detail occasionally begs for some generalization. (Ward makes one of his most important points in a footnote when he suggests the inadequacy of the concepts “new” and “old” worker for explaining the ways in which workers responded to change.) Women, despite their numerical predominance in the cotton industry, remain in the background, but Ward subtly delineates the traditional cultural relationships that kept women out of the sources. These quibbles are minor. This is a path-breaking study that will serve as a model for future investigation of labor and industry in the Soviet period and one that will be of great value for comparative working-class historians as well as Russian historians.

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ILYA PRIZEL. *Latin America through Soviet Eyes: The Evolution of Soviet Perceptions during the Brezhnev Era, 1964–1982*. (Soviet and East European Studies.) New York: Cambridge University Press. 1990. Pp. xiii, 253. \$44.50.

Now that the Brezhnev era and its aftermath have been ended by five years of *glasnost* and *perestroika*, scholars are beginning to be able to examine the years between 1964 and 1982 from a historical perspective. Ilya Prizel has made an attempt to do this with respect to official and semiofficial perceptions of Latin America during the Brezhnev years. Prizel approaches his subject in a fairly mundane way and with an organization that is logical if uninspired, but he does introduce some interesting considerations into his analysis of a subject that has not been rigorously studied previously.

Prizel breaks his study into four parts. The first two, which concentrate on Soviet perceptions of Latin America's global role and social structures, are the most substantive. Prizel is correct to emphasize the growing Soviet sophistication in analysis of Latin America during these years, and his stress placed on the role played by the journalists and academics who helped shape Soviet policy toward that region is certainly legitimate. One especially good example of the significance of academics is Anatolii Fedorovich Shul'govskii, a scholar associated with the Institute of Latin America whose work is respected both within the USSR and abroad. Indeed, since he cites him repeatedly, Prizel might have argued that Shul'govskii's analyses were the best—informed, most consistently if imaginatively Marxist, and most typical of overall Soviet views of Latin America during the Brezhnev era. Other Soviet analysts were rarely as perceptive as Shul'govskii, and U.S. politicians who read Soviet writings on Latin America would have recognized that Soviet policy toward the region was much less adventurous than numerous U.S. administrations gave it credit for being.

The book's third section, dealing with a few case studies within Soviet-Latin American relations during the Brezhnev era, is less satisfying. Whereas Prizel's discussion of the Soviet-Chilean relationship is a good summary of the countries' ties, his sections on Brazil and Argentina are little more than superficial commentaries, and his analysis of contacts between the USSR and Mexico lacks substance and is too brief to tell us much about either country.

Part 4, Prizel's conclusion, is concise and predictable. Indeed, as the author argues, there was a dramatic improvement in the quality and sophistication of Soviet analysis of Latin America during the Brezhnev years. But the region remained far away from immediate Soviet concerns, and a more sophisticated Soviet understanding of Latin America meant that the Soviet government was less likely to engage in activities in the area solely from political and ideological motivations that had no foundation in local realities.

The last section of Prizel's book, an “epilogue” on the Gorbachev era, attempts to bring up to date the perceptions discussed in earlier pages. Unfortunately, both its analysis and its predictions about future Soviet understanding of Latin America are

already outdated, add little to the book as a whole, and probably would best have been omitted.

Finally, it must be noted that there are numerous spelling errors that detract from the book's veracity. Corvalán is consistently misspelled "Corvolan," Maidanik repeatedly appears as "Maidonik," Podgorny becomes "Potgorny," and the Mexican oil company Pemex becomes "Penmex." Worse, Prizel adopts the frequent Soviet mistake of referring to the "continent" of Latin America when he clearly means to include Central America and Mexico as well. In the end, such errors have little to say about the substance of Prizel's arguments, but they do denote a sloppiness that leads the reader to suspect other details in the book.

On the whole, Prizel is to be lauded for attempting a study of a subject that deserves the attention he gives it. This is not a perfect book, but it does ask some interesting questions and provides at least some stimulating answers.

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#### NEAR EAST

MUHAMMAD A. DANDAMAEV and VLADIMIR G. LUKONIN. *The Culture and Social Institutions of Ancient Iran*. Translated by PHILIP L. KOHL. Assisted by D. J. DADSON. New York: Cambridge University Press. 1989. Pp. xv, 463. \$90.00.

With the publication of the present book, we now have available in English the first comprehensive work devoted to the history of Iran through the end of the Achaemenid empire since A. T. Olmstead's *History of the Persian Empire* (1948). The original Russian publication of Muhammad A. Dandamaev and Vladimir G. Lukonin's work (1980) used sources available up to the end of 1976. This volume was brought up to date as of 1986 for the English edition by Philip L. Kohl, who also edited the preliminary translation of D. J. Dadson. It is important to note this chronology because of the nature of the sources for the history of ancient Iran and the vigorous controversies over certain crucial events in that history.

The title clearly indicates the contents of the book. The three major sections are devoted to the early history and culture of the Iranian peoples of West Asia, the social institutions and economic structure of the Achaemenid empire, and Achaemenid culture, with an appendix that reviews the sources. The authors "have attempted to recreate an integral picture of the history of the Medes and the Persians based on archaeological remains, the rich cuneiform archives, and other sources" (p. xv). They have succeeded admirably in this aim by threading their way carefully through the primary sources and the differing interpretations of them, which have sometimes

been based on scanty or ambiguous information. Since 1948 a large amount of new data has been made available, thus outdating Olmstead's work. Many thousands of cuneiform tablets still remain to be read and published.

Because of the abundance of data, the authors are able to present a relatively detailed description of the economic and political structure of the Achaemenid state. They argue against the common belief in an unchanging feudalism that has been thought by Western scholars to characterize the social structure of ancient Iran, and they show how great social changes and a radical transformation in agrarian relations took place during Achaemenid times. The most controversial questions in the history of ancient Iran are the date of the emergence of Zoroaster, the nature of the religion of the Achaemenid kings, and the influence of the religion of the Magi. On all of these issues the evidence is reviewed carefully, and plausible hypotheses are offered. They set the date of Zoroaster at no later than the seventh century B.C., more or less midway between those who would have him contemporary with Darius I and those who believe that he lived around 1000 B.C. or earlier. The authors argue that Zoroastrianism remained unpopular among the Iranian people for the entire Achaemenid period, because of its abstract nature. The cults of Mithra and Anahita were widespread among the people, whereas the kings practiced various forms of Mazdaism, being influenced by both the belief of the Magi and the religion of the people. Zoroastrianism developed fully only after the Achaemenid period.

The book is elegantly produced, with maps and numerous illustrations. It should remain the standard source for the history of the Median and Achaemenid periods for a long time to come.

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M. REZA GHODS. *Iran in the Twentieth Century: A Political History*. Boulder, Colo.: Lynne Rienner or Adamantia, London. 1989. Pp. xiv, 296. \$35.00.

M. Reza Ghods's purpose in retelling the political history of Iran in the twentieth century is to underline the major role of Iran's "religious, patrimonial culture" in shaping the country's politics (p. 230). This culture, rather than class interests, he argues, provides the better analytical tool for understanding recent Iranian history. He thus intends his book as a corrective to the largely (but not exclusively) class-based approach that informs Ervand Abrahamian's massively researched and impressive *Iran between Two Revolutions* (1982). Class-based analysis, Ghods writes, underestimates the role of "patriarchal Islamic culture" and its influence on the masses (p. 230).

Ghods never quite makes clear what he means by Iran's religious (Islamic), patrimonial culture. This is



a major shortcoming because much of his analysis depends on a definition of the term. But it appears to encompass for him a certain deference to authority, a suspicion of ideas or policies that appear to threaten Islamic values, xenophobia, a tendency to personalize politics, and an inclination toward ethnic, ideological, and small-group factionalism. Religion, he suggests, has for the most part buttressed an authoritarian social and political system.

In developing his case, the author is most persuasive when dealing with the three radical, quasi-separatist movements of Iran's recent history—the movement led by Kuchik Khan in Gilan following World War I and the autonomy movements in Azerbaijan and Kurdistan following World War II. He has little difficulty in showing that both the Gilan republic and the Azerbaijan autonomy movement failed because of faction among the leadership and extremist policies that alienated the propertied and middle classes and offended the religious sensibilities of the peasants and lower classes.

Ghods has read widely in the sources. He writes with clarity and manages to provide a coherent account of a very long period. But there are problems with his study.

He is stronger on chronological political history than on analysis. In writing political history, he tends toward excessive detail. He sometimes relies heavily on the work of others, and he repeats a great deal of material amply covered elsewhere. He is also sometimes careless in the use of language. For example, he repeatedly refers to the Gilan, Azerbaijan, and Kurdistan autonomy movements as “revolutions.” These local, short-lived movements were nothing of the kind, certainly not commensurate with the constitutional revolution of 1906 and the Islamic revolution of 1979.

His sensible strictures against an exclusively class analysis of Iranian politics in this century notwithstanding, Ghods seems himself powerfully attracted by the class paradigm. Again and again he writes as if Iran's politics are a politics of classes. “The [constitutional] revolution,” he states, “was made primarily by the traditional *bazaari* middle class, but . . . other classes had a decisive influence on its course” (p. 30). He analyzes the political and economic conflicts in Iran in the 1960s and 1970s largely in class terms. “Upper class,” “upper middle class,” “lower middle class,” “the masses” are terms he uses repeatedly.

The focus of the book is somewhat uncertain. Ghods's real interest lies in the radical movements of the Left, but he also argues that the middle class, broadly defined, has been the driving force in Iran's oppositional politics. Moreover, Ghods cannot quite decide whether there is revolutionary potential in the peasantry and the “masses.” A major and sensible point that he seems to be making is that revolutionary parties of the Left have repeatedly failed because they advocated policies that ran counter to the beliefs and values of the middle classes and the common peo-

ple—thus, the failure of the Gilan and Azerbaijan autonomy movements.

Yet Ghods would also like to see both of these as popular, mass movements with revolutionary potential. He describes the peasantry in Gilan in 1919–21 as “non-revolutionary” and “counterrevolutionary” (p. 82), but he also labels the abortive Gilan republic as a “peasant nationalist revolution.” Concerning Azerbaijan in 1945–46, he argues both that the leaders of the Azerbaijan Democratic party pursued policies too radical for the middle and lower classes and that “the revolutionary masses' zeal” (p. 174) was dissipated for want of a cohesive ideology. In short, this is a readable political history with a blurred analytical framework.

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SELIM DERINGIL. *Turkish Foreign Policy during the Second World War: An “Active” Neutrality*. New York: Cambridge University Press. 1989. Pp. 238.

The role of Turkey as a neutral in World War II and its possible entrance into that conflict have been dealt with by numerous authors. Selim Deringil, associate professor at Bogaziçi University in Istanbul, has written an account that looks at the issues from a perspective close to the official position of the Turkish government at the time. He could not use Turkish archives but had access to the unpublished memoirs of Numan Menemencioglu, a key figure in the events described; he has used British archival materials; and he has relied heavily on Turkish newspapers and other publications. Neither German nor American archives were used, and some of the best-known works on the subject, such as Lothar Kreckler's *Deutschland und die Türkei im Zweiten Weltkrieg* (1964), Frank G. Weber's *The Evasive Neutral War* (1979), and especially Zehra Önder's *Die türkische Aussenpolitik im Zweiten Weltkrieg* (1976), were not consulted. Until access to Turkish archives is permitted, Önder's study will remain the standard work on the subject, although Deringil's book provides useful insights into Turkish policy.

The author sets the stage with a very useful summary of the backward economic and military situation of Turkey in the interwar years. He shows how the loss of the Greeks and the pushing out of other non-Turkish people drove the economy backward, not forward, and how Germany exploited this situation to become the country's largest trading partner. He explains the central role of İsmet İnönü in the determination of Turkish policy on all major subjects and provides examples of the way İnönü manipulated men and issues. Although the author shows no appreciation of the fact that it was Germany that, after Italy, most threatened Turkish independence, he does stress how one heritage of World War I was a



concern over the extension of German influence in the country.

Deringil follows the windings of Turkish policy through the years of the war, barely touching on the country's territorial aspirations (pp. 140–41) and emphasizing its interest in a compromise peace. Unwilling to see the possibility that the Allies were defending the independence of Turkey—which the Axis was eager to terminate—he extols Turkey's clever profiting from its sale of chrome ore to the highest bidder.

Deringil's view is that the Turkish government was never actually willing to enter the war, an interpretation not shared by all who have studied the subject. Certainly he is correct in showing that Winston Churchill's dream of enticing the country into the war was always just that, that this fantasy was related to his fixation on the Gallipoli campaign of World War I, and that any chance of its realization vanished with the British disaster in the Aegean campaign of 1943. Deringil shows that Churchill was prepared to see the Soviet Union control the Straits during the war and shifted policy on this point very late—an interpretation that fits with Churchill's general policy of preferring extensive concessions to Moscow from 1939 to 1944. Readers will find some errors and peculiarities in the book, but they will be grateful to the author for an explication of Turkish policy that, if not officially sponsored, does place that policy in the best possible light while tracing it in a clear and succinct manner.

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## AFRICA

CATHERINE COQUERY-VIDROVITCH. *Africa: Endurance and Change South of the Sahara*. Translated by DAVID MAISEL. Berkeley and Los Angeles: University of California Press. 1988. Pp. x, 403. \$35.00.

Catherine Coquery-Vidrovitch has assembled a wide selection of findings from twenty years of social history research and travel throughout Africa. The various colonial territories of sub-Saharan Africa combined a kaleidoscope of cultures and political structures making it impossible to find a single common tradition on which to build historical interpretations. Only an extremely knowledgeable and well read scholar could hazard a synthetic reconstruction of historical materials on the middle ground between microscopic monographs and macroscopic surveys. Coquery-Vidrovitch's work is neither a survey, nor an anthology, nor a journalistic overview à la Sanford Ungar's *Africa* (1985) or David Lamb's *The Africans* (1982). Going beyond the more popular *African Genius* (1970) and *Let Freedom Come* (1978) by Basil Davidson, this personal selection illustrates four major themes: population growth, the genesis of political power and the state, the crisis of the peasantry, and the formation of an urban proletariat. The selection

is woven together into a still very readable whole by this creative and encyclopedic scholar. Only two relatively recent works in English offer similar tours de force. These are Bill Freund's *Making of Contemporary Africa* (1984), which examines recent African history with an emphasis on the forces of production, and Patrick Manning's *Francophone Sub-Saharan Africa, 1800–1985* (1988).

Beyond its obvious value as a survey of much scholarship and interpretation in African history, this book performs another function. Coquery-Vidrovitch is a favorite of "progressive" Africanist scholars in the United States, and this work offers English-speaking scholars an excellent opportunity to experience one of the primary Africanist representatives of the *Annales* school of historiography. This school places heavy emphasis on the ecological and material determinants of cultural development and looks beyond the relatively short-term, narrowly focused periodization and causation of much Africanist writing in English to trends and continuities of longer terms. Coquery-Vidrovitch reminds the reader that one's categories of analysis are the determinants of one's findings. Those whose historiographic training has been limited to political and nation-centered history will find this book a challenge. Its breadth of perspective and its reminders of the importance of categories, frameworks, perspectives, and levels of significance for the academic analysis of things African make this a truly important book.

The original French edition, *Afrique noire: Permanences et ruptures*, was published in 1985. Although the author states that this "American" edition records some developments since 1985, it is obvious from the relatively few items in the bibliography after 1986 and verb tenses throughout (for example "Sankare [who was killed in 1987] is a former Catholic choir-boy" [p. 107]) that this is more a direct translation than a second or U.S. edition. The use of some direct translations ("I have known the village hut . . . and the chance hazards of the bush" [p. ix]) lends a certain archaic charm for U.S. scholars who have been trying for decades to overcome myths of inferiority by avoiding such loaded terminology (on "hut," see Evelyn Jones Rich, "Mind Your Language," *Africa Report*, September–October 1974). A number of sweeping generalizations border on what North American progressives generally term "racism." For example, "Africans . . . display neither a European sense of initiative, nor an Asian willingness to work" (p. 318) might be acceptable for sophisticated U.S. Africanists, but one should give careful consideration to such views before uncritically assigning this book to undergraduate North American students.

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C. R. PENNELL. *A Country with a Government and a Flag: The Rif War in Morocco, 1921–1926*. Wisbech, U.K.:

Menas; distributed by Kingston, Clifton, N.J. 1986. Pp. xiv, 270. \$35.00.

Between 1921 and 1926 the war in the Rif mountains of northern Morocco brought international notice to an obscure tribal leader, Muhammad bin 'Abd al-Karim al-Khattabi, who delivered a humiliating defeat to a Spanish army at Anwal in July 1921, then kept the Spanish at bay for four more years. That a rag-tag coalition of guerrilla fighters from an impoverished corner of "Spanish Morocco" could halt Spain's military advance and even take on France (which 'Abd al-Karim did in April 1925 when he crossed into "French Morocco") demonstrated not only the force of anticolonial resistance but also an energetic leadership and a resolute quest for self-determination. The Rif war ended with the defeat of 'Abd al-Karim and his followers by combined French and Spanish armies. During its course, however, the war exposed the failures of colonial policy in Morocco and provided a rallying point for anti-imperialist and antimilitarist sentiment in both Spain and France. Perhaps most important, the Rif war changed the Rif itself.

C. R. Pennell documents the transformation that 'Abd al-Karim and the Rif war brought to the politics and society of northern Morocco. Pennell argues that 'Abd al-Karim sought to create an independent and centralized authority in the Rif—a country with a government and a flag—where one had not existed before. As described by Pennell, 'Abd al-Karim's talent and accomplishments were impressive, in particular his ability to use the traditional to fashion something new. His rise to power came from the skillful use of family, clan, and tribal connections, pulled tightly together by the demand for resistance to the Spanish, expressed as a call for jihad or religious war. The mission to safeguard Islam—and by implication to firm up both the faith and the faithful by strict adherence to the requirements of the *shari'a*—allowed 'Abd al-Karim to appoint officials, make laws, levy taxes, impose penalties, and raise an army. Backed up by the force of his own tribe, the Banu Waryaghal, he succeeded in establishing a unified political and military control of the Rif, supported by a communications grid of roads and telegraph lines. Upholding the law and maintaining the peace now became the government's domain rather than that of the clan or tribal council. As a result, Rif society was touched as it had not been in the past.

It may be that 'Abd al-Karim and his ephemeral Rif Republic were not the precursors of Moroccan nationalism, for this was thought and action on the local level. But he was no retrograde primitive rebel either. He attempted to employ the ways and means of modern times and met with some success. As Pennell makes clear, however, the presence of 'Abd al-Karim's government and the very pervasiveness of its control called forth a resistance from among the tribes. This was surely a factor in its demise. The

great merit of Pennell's fine study is to recall 'Abd al-Karim's importance as an innovator among all those who resisted and lost.

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MICHEL ABITOL. *The Jews of North Africa during the Second World War*. Translated by CATHERINE TIHANYI ZENTELIS. Detroit: Wayne State University Press. 1989. Pp. 212. \$29.95.

This excellent book by Michel Abitol provides a subtle and sensitive analysis of the travails of North African Jewry under the Vichy regime and of the tortuous path of their liberation with the changing fortunes of war.

The line of defense adopted by the apologists of the Vichy regime has consistently been that anti-Semitic legislation was imposed on the French by the occupying German power. Recent historiography, especially the definitive work on Vichy and the Jews by Michael Marrus and Robert Paxton, has shown conclusively that the impetus for this legislation came from indigenous French anti-Semitic forces and preceded any German initiative. Abitol demonstrates skillfully how this legislation was made to apply at the same time to French North Africa and was implemented with great ruthlessness by the French civil service in place. Furthermore, the laws that removed or significantly reduced the number of Jews in education, in the economy, and in the liberal professions were also accompanied by the repeal on October 7, 1940, of the Crémieux decree of 1870, which had granted French citizenship to the Jews of Algeria. The repeal of the decree had long acted as the central tenet of the anti-Semitic creed of the French *colons* and was the culmination of their longstanding demands to limit the place of the Jews in European Algerian society. Abitol also demonstrates the great zeal with which the French authorities applied anti-Semitic laws in Morocco and Tunisia where the Jews had in their overwhelming majority remained subjects of the Muslim rulers and were not French citizens. The local Muslim authorities sometimes acted as buffers but could not intervene in any effective manner. Paradoxically, the Spanish and Italian governments, officially friends and allies of the Germans, often acted as the protectors of the Jews in Morocco and Tunisia.

The Anglo-American invasion of North Africa in November 1942 did not, in fact, lead to the immediate removal of the Jewish disabilities. The local French military and civil leadership, still pro-Vichy although now anti-German, was loath to remove the anti-Semitic legislation. It was only after tardy pressure from the Americans and the ascendancy of the Gaullists that the last vestiges of these measures were repealed and the Crémieux decree reinstated.

Abitol places this sorry episode in the history of North African Jewry and that of France within the larger context of the evolution of the destruction of European Jewry during the war years. The fate of North African Jewry was, of course, ultimately linked to the victory or defeat of the Germans. Nevertheless, the story presented so eloquently in this book, illuminating the peripheries of the Holocaust, also throws light on the crucial role played by local forces who used the occasion of the *divine surprise* to push for their anti-Semitic aims. The presence and success of such elements constituted an important factor in the facilitation of the task of the Nazis in the elimination of European Jewry. It is clear that the local French administrations would not have acted as a brake or as an impediment if the Germans had attempted to deport the Jews. If it had not been for the relatively short duration of the German presence in the area and the lack of railway links to the death camps because of the Mediterranean, the fate of North African Jewry would have mirrored that of its counterparts in Europe.

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JUHANI KOPONEN. *People and Production in Late Precolonial Tanzania: History and Structures*. (Studia Historica, number 28.) Helsinki: Finnish Historical Society. 1988. Pp. 434.

In this tour de force of criticism and synthesis, the Finnish scholar Juhani Koponen provides us with the foundations for a people's history of mainland Tanzania. He identifies the parameters of his task with an initial chapter on the historiography of Tanzania, within which he highlights for sustained attention the important and essentially contrary contentions of Helge Kjekshus, *Ecology Control and Economic Development in East African History* (1977), and John Iliffe, *A Modern History of Tanganyika* (1979).

People's history as pursued here links questions of population levels and material culture and, although not ignoring the implications of an increasingly extensive and intensive external trade, is more preoccupied with the potentials and evidence of accumulation within communities of various degrees of hierarchy and wealth. Koponen agrees neither with Kjekshus that colonial intervention dislocated the admirable ecological control that had previously secured a general well-being nor with Iliffe that early colonialism brought improvement.

The author makes it plain that arguments are as important as data in the reconstruction of a chronologically informed base line for a social history of Tanzania in the twentieth century. He marshals an impressive array of published works, including early ethnographies and modern studies of the country as

a whole and in its highly varied localities. He also invokes a range of theorists whose ideas might help to illuminate the analysis of the late precolonial and, indeed, the colonial and recent experience of Tanzanians. The theoretical references are to such thinkers as the economist Amartya Sen and the anthropologists Maurice Godelier and Pierre Gourou. Although the book does not acknowledge the French *Annales* school, it reflects its spirit.

Ultimately, although the volume adds significantly to the historiography of Tanzania, it must also be seen as a remarkably graceful, accomplished, yet still preliminary investigation of how ideas and evidence intertwine. For graduate students interested in pre-industrial cultures and people, the volume serves to exemplify the art of selection, drawing out salient components, looking forward to overall generalizations, and taking into account the myriad details and specificities of local life as presented in the raw primary evidence. Readers may be anxious for more positive findings or at least an example of how the explorations of local history, as advocated by the author, might be assembled into a textured whole. To this end, people's history must be welded once again to political processes of accumulation. A dynamic rendering of political economy as seen, felt, and participated in from below will be a necessary further step in straddling history and structures in late pre-colonial and modern periods alike.

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MARSHALL S. CLOUGH. *Fighting Two Sides: Kenyan Chiefs and Politicians, 1918-1940*. Foreword by JOHN LONSDALE. Niwot: University Press of Colorado. 1990. Pp. xxv, 246. \$29.95.

Marshall S. Clough's book examines the growth of political activity among the Kikuyu in the Kiambu district of Kenya in the interwar years. With their proximity to Nairobi and many white settler estates, the inhabitants of Kiambu were the first Kenyans to weather the full force of British colonialism: land alienation, labor recruitment, missionary evangelism, and Western education. Predictably, they also became the first to establish modern political organizations to defend their interests. Clough's principal intent in this book is to show how the vagaries of the colonial order both created and accentuated divisions within Kikuyu society—between elders and youths, chiefs and subjects, landholders and tenants, traditionalists and Christians, and peasants and urban workers—that were manifested in the rise and rivalries of the Kikuyu Association (KA), the East African Association (EAA), and the EAA's successor, the Kikuyu Central Association (KCA).

Clough closely follows the path pioneered by Carl Rosberg and John Nottingham in their classic work, *The Myth of "Mau Mau"* (1966). He broadly confirms

their analysis of the social and personal factors that distinguished and divided the KA, the EAA, and the KCA. Apart from the empirical richness of his narrative, Clough's principal contribution lies in his effort to retrieve the Kikuyu chiefs from the condescension of the label "collaborators." This identification, he argues, obscures the complexities of their position, which required them to mediate between the varied demands of officials, settlers, and missionaries and the equally diverse pleas of an increasingly stratified Kikuyu population. It was the Kiambu chiefs, particularly a group of four young, talented, and progressive Christians who acceded to chieftaincies in the early 1920s, who dominated the Kikuyu Association, the first organization to give political voice to Kikuyu interests. Although the KA was cautious in its policies, elitist in its character, and determined to destroy its populist rival, Harry Thuku's EAA, it was far from inconsequential as a force for the furthering of African interests. Clough shows that its leaders fought tenaciously to protect indigenous land rights, certainly the most potent of Kikuyu grievances, and often voiced vigorous objections to colonial policies. The best that the chiefs could do, however, was lead a holding action against settler encroachments, and they gave little consideration to the special problems of squatters on white farms, migrant laborers in Nairobi, and the *ahoi* (tenants) who often worked their own land. Their position consequently became increasingly untenable. Under pressure from all sides, the KA and its chiefly sponsors lost the initiative to the KCA, especially during the female circumcision controversy of 1929 and 1930—hence, the title of the book, which echoes Rosberg and Nottingham's observation that "the growing legacy of bitterness and conflict made it increasingly difficult for the chief to walk this tight-rope between nationalism and collaboration" (*The Myth of "Mau Mau,"* p. 83).

Clough portrays Kikuyu politics between the wars as very much an enterprise of competing elites, and he skillfully traces the political activities of chiefs, lineage leaders, mission-educated clerks, and other figures of prominence within Kikuyu society. Yet it is clear that the incipient mobilization of the Kikuyu masses also occurred during these years, notably through the efforts of the EAA in the early 1920s and the KCA at the end of the decade. Clough's book offers little insight into that subterranean process of mass politicization that was so important to the rise of Mau Mau. His book does, however, restore the chiefs to a prominent place in the political history of Kenya, and it thereby contributes to our understanding of the origins of the social conservatism that characterized the Kenyatta regime in independent Kenya.

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JOHN HIGGINSON. *A Working Class in the Making: Belgian Colonial Labor Policy, Private Enterprise, and the African Mineworker, 1907–1951*. Madison: University of Wisconsin Press. 1989. Pp. xix, 307. Cloth \$40.00, paper \$18.75.

In the recent historiography of Africa, the development of working-class consciousness has been all too frequently approached from a theoretical perspective, but, with the exception of a few of the most talented South Africanists such as T. Dunbar Moodie and Charles van Onselen and the highly focused monographs by Bogumil Jewsiewicki and Johannes Fabian, the cultural changes involved in adaptation to salaried employment have received little attention. John Higginson's study of the African copper miners is therefore particularly welcome, based as it is on an exhaustive survey of company, local government, and diplomatic archives on three continents.

His sources do, however, raise a historiographical problem in that few of them contain documents written by and for Africans. Nonetheless, his citations to judicial cases originally collected by Jean-Luc Velut, provide personal vignettes unparalleled in the historical literature. Indeed, one might well argue that he understands the behavior of African workers better than that of the European managers.

His hypothesis is that copper industry workers successfully resisted the attempts of "private industry and the Belgian colonial government . . . to proletarianize Africans without creating a working class" (p. 3). That is to say, workers, through the "persistence of pre-trade union forms of organization" (p. 11)—mutual aid societies—came to constitute "a social entity capable of pursuing interests that could threaten those of the mining company and the state" (p. 16). The nagging question that remains in my mind is the extent to which the interests of the miners were in continuous opposition to those of the company. Indeed, Higginson admits that "workers did occasionally consider their interests incompatible with those of the company" (p. 13).

His argument is laid out in three units: the first describes the Union Minière du Haut-Katanga, the copper company, from its first African operations in 1907 until the installation of the company's labor stabilization program in 1928; the second follows the development of worker consciousness in both cities and mining camps from 1919 to 1939; the third describes the clash between workers and the company and state between 1937 and 1951, focusing on the strike of 1941 and the insurrection of 1944, both unsuccessful. His conclusion is that the movements failed because of "restrictions imposed on the workers," which he defines as limitations on "the right to express their political and social opinions freely" (p. 214). Thus, they "had been defeated, but they had resisted the mining company's efforts at socially engineering their personalities" (p. 215).

All of this is true, but the company went in for



more than just social engineering. Its management was in the business of producing copper at the lowest possible cost. In the early years, this goal was achieved at the cost of enormous suffering and death among Africans who had been dragooned into the work force. Later, however, Africans sought employment in the mines—as they still do, because conditions there are much better than elsewhere. It is all very easy to criticize industrialization, but one must also consider the available alternatives. In this anti-industrial bias, however, Higginson shares a viewpoint held by most of the writers of the last generation. His hostility to management is only reinforced by occasional misinterpretations of the evidence: during the late 1930s, food allocations supplemented rather than reduced worker salaries (pp. 179–80); and Fernand Grévisse, the liberal administrator, entered, not left, Katanga in 1934. Inaccuracies aside, Higginson has provided a generation of readers with an important guide to the world the African copper workers made.

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RANDALL M. PACKARD. *White Plague, Black Labor: Tuberculosis and the Political Economy of Health and Disease in South Africa*. (Comparative Studies of Health Systems and Medical Care.) Berkeley and Los Angeles: University of California Press. 1989. Pp. xxii, 389. Cloth \$40.00, paper \$15.95.

This is a fine study of the history and current status of one of the major health problems confronting South Africa. Randall M. Packard documents the rise of tuberculosis (TB) in a colonial society that was marked by both the most extreme degree of racial discrimination and, somewhat ironically, the highest rates of urbanization, industrialization, and economic growth on the African continent.

As the subtitle indicates, the author takes a broad approach to this very social disease. His approach is generally useful and successful, although Packard exaggerates the “capitalist” factor in South Africa’s expanding mines, cities, and factories. Although the peculiarities of South Africa’s racial situation and its economic development under what might be broadly defined as capitalism with a strong admixture of state-run enterprises are certainly relevant, the basic processes influencing TB were urbanization and industrialization, not the capitalist system. It is clear from events in Eastern Europe and the USSR that these processes can have adverse health effects under other forms of economic organization. Socialist industry also pollutes.

Packard argues that TB may have existed in South Africa on a small scale long before the economic transformations of the late nineteenth century, so the disease did not strike a “virgin soil” population. This

view may be correct, but, as he notes, many Africans were immunologically naive. European immigrants, including TB sufferers seeking relief in South Africa’s sunny climate, helped spread the disease. Harsh working conditions, overcrowded housing, and very inadequate diets led to a TB epidemic among Africans attracted to the mines and cities after the 1880s. Victims, often fired because of illness, spread tuberculosis back to the countryside. Deteriorating conditions in increasingly impoverished rural labor reserves forced more migrants to seek city and mine jobs, where the cycle of low pay and poverty led to more disease. Low-wage migrant labor, so crucial to the South African economy, was a powerful stimulus to the spread of TB and other diseases.

Packard argues convincingly that the basic cause of epidemic tuberculosis among the African population was poverty, both urban and rural. Because cheap black labor was essential to all segments of the economy and Africans were excluded from political participation, neither private enterprises nor the government was pressured to make serious reforms. Ameliorative measures were taken, but improvements were usually local and temporary. Lasting results were impossible within the narrow scope of medical action or urban reform. The mining industry did improve conditions for its workers during the 1920s and 1930s, surprisingly quickly and perhaps with more beneficial results than Packard allows. Governmental agencies conducted numerous valuable studies, but attempts to improve housing and food were woefully inadequate. Indeed, a major solution for both mine owners and white officials was to banish sick Africans to the supposedly healthy reserves and juggle political boundaries to exclude TB sufferers from “white” areas and hence from the official record. The most extreme version of this neglect was the Bantustan policy of the 1960s.

This is an important book for students of South African history as well as for those primarily interested in health issues. Packard skillfully uses the relatively abundant data collected by a wealthy state. Information is deficient for rural areas, as in most countries, and it is often difficult to establish populations at risk for determining ratios, but such methodological problems are well handled.

My criticisms of this excellent book are minor. Packard may underestimate the value of modern drug therapy. Despite problems such as drug-resistant bacteria, mortality has fallen significantly since the early 1950s. I agree, however, the morbidity is probably rising and that therapy alone can never solve the problem. The author minimizes the serious issue of steady, rapid demographic growth. Comparative material from studies on other African countries would have strengthened the work. Colonial West Africa had some interesting similarities and differences, and the experiences of African miners in the



Rhodesias or in the Belgian Congo could have shed light on the South African situation.

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## ASIA

JAMES T. C. LIU. *China Turning Inward: Intellectual-Political Changes in the Early Twelfth Century*. (Harvard East Asian Monographs, number 132.) Cambridge: Council on East Asian Studies, Harvard University; distributed by Harvard University Press. 1988. Pp. ix, 216. \$23.00.

The book under review has been a long time evolving. Many of its key concepts have been set forth previously by the author in academic conferences and journals over the last thirty years. Raising the issue of why traditional China, at one time the most technologically advanced country in the world, eventually stagnated and became a poverty-stricken, underdeveloped country, James T. C. Liu hypothesizes the existence of a powerful state that, under a ruling ideology, stifled all developments threatening the status quo. Going a step further than other analysts, he describes the process whereby an orthodoxy in the ruling ideology was formed.

In tracing the tortuous path followed by the Ch'eng-Chu school of neo-Confucianism, which became the ruling ideology, Liu commences with an account of the lively intellectual scene during the heyday of the Northern Sung period. Next he deals with the chilling effect of the reforms of Wang An-shih. Finally came a series of traumatic events that killed off the tender sprouts of free inquiry and original thought: the Jurchen invasion, the collapse of the Northern Sung, and the establishment of the Southern Sung by Emperor Kao-tsung. In the court of Emperor Kao-tsung, which struggled for survival, the cause of reform was discredited because it was identified with the legacy of Wang An-shih and blamed for the Northern Sung debacle. The only intellectual-political orientation for men of principle was moral conservatism. These moral conservatives, however, fared badly after their leader, Chao Ting, was sacrificed by the emperor on the altar of expediency. The life and work of Chao Ting and of his associates, nevertheless, inspired generations of like-minded scholars who strove hard to remedy the shortcomings of their Confucian heritage by developing a comprehensive metaphysics as the underpinning of their value system, hence the name "Transcendental Moralists." Through their scholarship, their teaching, and their involvement in community service, they gained prestige until finally the court had to reckon with them. By the mid-thirteenth century, under pressure from the Mongols, the court decided to tap the prestige of these scholars by honoring their leaders and making their commentar-

ies on the classics the basis of the civil service examinations.

To list all that is meritorious in the book is impossible. The survey of the Northern Sung intellectual landscape is superb; so is the discussion of court politics during the reign of Kao-tsung. Liu is probably the first modern scholar to recognize the pivotal significance of Chao Ting in the political-intellectual history of the Southern Sung. His account of the background factors for the triumph of the Ch'eng-Chu school is convincing.

In a work of such scope and magnitude, minor blemishes are inevitable. Liu contends that the transition period between the two Sung saw the redirection of Confucian activism away from the outer realm toward the inner realm and that the repudiation of Wang An-shih's legacy ruled out the route of reform so that the only viable stance for high-minded scholars was conservatism, that is, a scrupulous adherence to the established institutions of the founding emperors of the dynasty. These observations reflect the keen insight of the author. Nevertheless, the empirical evidence that he adduces in support of his argument is slim, and no attempts have been made to explain away seemingly contradictory phenomena. Emperor Kao-tsung's reign was not a time of tranquility but of turmoil, and, instead of holding fast to time-honored traditions, the court had to sanction radical departures from the established pattern in fiscal, military, and field administration in order to survive. Could these developments be construed as reforms? What were the attitudes of scholars toward them? Undoubtedly, repudiation of Wang An-shih's legacy did not rule out further institutional evolution nor prevent high-minded scholars from directing their studies toward the outer realm of institutions (for example, Ch'en Liang and Yeh Shih).

Minor blemishes notwithstanding, Liu has made a major contribution to Chinese intellectual history; he sets the agenda and defines the issues for further research that those of us who follow in his footsteps are challenged to pursue.

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YOUNG-TSU WONG. *Search for Modern Nationalism: Zhang Binglin and Revolutionary China, 1869-1936*. (East Asian Historical Monographs.) New York: Oxford University Press. 1989. Pp. xiv, 233. \$29.95.

Zhang Binglin is one of those revolutionary figures who does not fit conventional molds. He is most famous as an anti-Manchu propagandist before the Chinese revolution of 1911—a proponent of what both Charlotte Furth and Chang Hao have termed "ethnic nationalism." Yet he was also a classical scholar, a student of Mahayana Buddhism, a reader of German idealist philosophers, and a contentious individual who often quarreled with his political

associates. The overarching image is of a racially motivated revolutionary who became a conservative scholar seeking to protect China's national essence against Western-inspired iconoclasm. Young-tsu Wong offers another perspective in this study. Wong argues that Zhang was not a cultural conservative but a modern nationalist who believed that cultural distinctiveness and historical memory were crucial to national identity. His nationalism was, moreover, shaped more by anti-imperialism than anti-Manchism.

This is a solid political biography. It seeks to extricate Zhang's record from prejudices originating in political disputes with Sun Yatsen, Jiang Jieshi, and other revolutionary leaders. It focuses on Zhang's political thought and activities, and it looks at how he reacted to the changing political context from the 1890s to 1936. Wong understands the personalized divisiveness of Chinese politics and masters Zhang's sometimes daunting prose. He uses new documents, draws on the memories of some of Zhang's old students and relatives, and benefits from contacts with scholars in China who are reappraising Zhang.

An account of Zhang's entire life provides a broader perspective and presents new information neglected by scholars concentrating on famous aspects of his early career, such as his trial and imprisonment for sedition in 1903 and his exposition of revolutionary ideology as editor of *Minbao* in Tokyo from 1906 to 1908. We learn about his frustrating effort to get support for plans to resist foreign penetration of Manchuria as frontier commissioner in Yuan Shikai's government during 1912, find out more about his role in the provincial self-government movement of the 1920s, and acquire more details of his generally acrimonious relationship with the Guomindang leaders. Wong is certainly an advocate of Zhang but gives generally balanced and reasonable accounts. The focus on Zhang sometimes magnifies the importance of his activities in the large political picture, however. Despite his moral and intellectual contributions, Zhang never acquired political power in the revolutionary movement. After the early 1910s, his impact on politics seems more peripheral than Wong indicates.

The treatment of the issues of nationalism and conservatism is more controversial. Wong convincingly argues that Zhang was primarily motivated by opposition to imperialism; his anti-Manchism arose from the conviction that the Qing government could not protect China and was abandoned when the dynasty fell. The question of Zhang's conservatism still remains problematic. The conservative label has never quite fit a man who shared so many ideas with radicals before 1911, was so familiar with Western works, and set forth views resonating with socialism. Wong's argument, however, becomes strained when, for instance, he plays down Zhang's hostility to vernacular literature. Zhang may have been, as Wong

suggests, a "cultural pluralist" who believed all nations should have their own cultural identities. But Zhang's determination to preserve the heritage in the Chinese classics and histories appears conservative in the context of the iconoclasm of the 1910s and 1920s and was also out of step with the attraction of many intellectuals to anarchism, socialism, and communism from the May Fourth period onward. This study gives us new facts and new interpretations, but a final appraisal of Zhang will have to analyze his social networks more fully and integrate the political focus of this book with analyses that also include his philosophy and scholarship.

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SULAMITH HEINS POTTER and JACK M. POTTER. *China's Peasants: The Anthropology of a Revolution*. New York: Cambridge University Press. 1990. Pp. xiv, 358. Cloth \$49.50, paper \$14.95.

About half of this book outlines the post-1949 history of Zengbu Brigade in Guangdong Province, China, and the other half talks about it, mostly in general terms.

The history of the brigade takes the reader through the foundation of the People's Republic, the land reform, the commune, the Cultural Revolution, and the rise of private enterprise in the last decade. The chapters that talk about the brigade in general terms deal with such subjects as the place of emotion in social life, marriage and family, birth planning, the lineage and the collective, the party, and the peasant's position in modern China's social order. Despite the fieldwork conducted by Sulamith Heins Potter and Jack M. Potter, this book tells us little that is not already known from readily accessible secondary works. A thread of an argument runs through that finds collectivization disappointing and takes heart in the persistence of the lineage. One does not necessarily have to quarrel with such an argument, but one does wonder now and then what the authors mean.

The difficulty the reader has to face is the often-shoddy ethnography. For a first example, the authors report that the production teams at Zengbu were made up of people from different lineage segments. What can such a statement mean, however, when it is also clear from the authors' description that the teams consisted of people living in the same neighborhood and that each neighborhood consisted of clusters of close agnates? Did the authors ever obtain or compile a name list for any team and check systematically how each household related to the lineage? Are they reporting an observation or hearsay?

For another example, the authors argue that local cadres became demoralized when private enterprise became official policy in 1983. How does this statement square with the argument that the cadres were also the people who in 1984 succeeded in taking

advantage of the same policy to engage in private enterprise? No ethnography is cited for either claim. Are we dealing with only the anthropologists' subjective impressions?

For a third example, the book gives a fairly detailed account, one supposes from firsthand observation, of an election of production team leaders in 1979. There was a secret ballot, and participation was opened to all adults in the team. The authors conclude from the description that "government at the team level . . . was democratically chosen: the peasants had the important right to elect their immediate work supervisors" (p. 102). Really? The authors agree that the slating of candidates for election was constrained and that the brigade leadership did intervene in some team elections, even though they argue that the pressure exerted was informal. The reader should turn, however, to the chapter on the party, which assigned cadres to oversee each village in the brigade, and one wonders if the party did not meet before the election to discuss the slating. What did the anthropologists do in the field to observe power in action?

Although the authors find the elections "democratic," they also find the bureaucracy "feudal" and the party "Leninist." The incongruity in these terms reflects muddled thinking.

This is a very uneven book. The specialists will want to look at short extracts here and there. The critical general reader will be bewildered.

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TESSA MORRIS-SUZUKI. *A History of Japanese Economic Thought*. (Nissan Institute/Routledge Japanese Studies Series.) New York: Routledge, for the Nissan Institute for Japanese Studies, Oxford, U.K. 1989. Pp. vii, 213. \$35.00.

This book fills an important gap in the knowledge of Japan available in English-language studies. Although a number of Japanese economists have made important contributions to economic science through books and articles published in the West, what they have been saying about the economic problems of Japan back home is largely a closed book to Western readers. This volume reveals surprising characteristics of thoughts and actions of Japanese economists as concerned citizens and opinion leaders of their own country.

Chapter 1 deals with premodern Japanese economic thought during the period when Japan was a closed feudal state with a rigid sociopolitical stratification and an occupational structure aligned to a structure of inherited status. Even so, the "instinct to truck and barter" (to borrow from Adam Smith) kept markets alive and expanding. This created economic problems worthy of scholarly attention.

The etymology of "economics" is important in this connection. Quite unlike the Greek roots of "economy," the Japanese counterpart of economy—*keizai* (short for *keikoku saimin*)—originates in Chinese Confucian ethics and means, in literal translation, "administering the nation and relieving the sufferings of the people" (p. 13). Though weakening over the years, the problem-centered discourse implied by *keizai* has continued to be a distinct focus of Japanese economic thought to this day.

After the opening of Japan to the world, all kinds of Western ideas vied for Japanese attention and allegiance (chap. 2). *Keizai* as defined above apparently served as a sorting device for a selective absorption of these Western imports. First, liberal political economy enjoyed a brief popularity. In the course of the Industrial Revolution, the "sufferings" of factory workers directed the attention of the Japanese to German economics with emphasis on "social policy."

Chapter 3 demonstrates, however, that "social policy" was neither rigorous enough to satisfy scholarly enthusiasm nor effective enough to produce advocated remedies. The hard-hitting idealists turned to Marxism. Two groups (the Kōza and Rōnō schools) emerged with two different versions of Japanese capitalism. The debates between them left enormous volumes of research, at a fairly high level of analytical sophistication, which had just begun to attract Western scholars' attention in the 1980s. Japan's defeat in World War II and the radical reforms at the hands of the Allied forces occupying Japan vindicated the Marxists' analyses of the ills of the country. For fifteen years or so after the war, Marxist economics ruled Japanese academics (chap. 4).

At the same time, the war-torn Japanese economy needed reconstruction and development. The required analytical expertise was supplied by "modern economics" (neoclassical resource allocation, Keynesian demand management, and economics of growth and development). An interesting twist in this respect was the rise of *kancho ekonomisuto* (bureaucratic economists, chap. 5). Modern economists and *kancho ekonomisuto* pulled off Japan's "miracle growth" of the 1960s, while Marxism was eclipsed in the shadow of renewed capitalist development.

Large-scale industrialization at a breakneck speed brought about unexpected problems such as environmental pollution, and "modern" economists began to doubt the relevance of their expertise. The final chapter describes how economists have deserted economics in droves in favor of broader cultural, social, or political paradigms that downgrade the economy and economics to a minor role.

This book is a very readable historical sociology of economic knowledge. By offering remarkable portrayals of generations of Japanese economists, it stimulates the appetite for knowing more about their scientific work. This study should be followed by

another on the history of "economic analysis" in Japan.

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JOSHUA A. FOGEL. *Nakae Ushikichi in China: The Mourning of Spirit*. (Harvard East Asian Monographs, number 139.) Cambridge: Council on East Asian Studies, Harvard University; distributed by Harvard University Press. 1989. Pp. xv, 313. \$23.00.

Prewar Japan's Sinologists made significant contributions to modern scholarship in Chinese history by combining their competence in classical Chinese with their passion for mastering modern Western concepts. As intellectuals, they were inevitably caught up in a national soul searching concerning Japan's relationship with China and the West. This well-written biography of Nakae Ushikichi (1889–1942), a self-taught Sinologist who lived most of his life as an expatriate in Peking, tells a lot about intellectuals in prewar Japan and their responses to the crises in Sino-Japanese relations. Joshua A. Fogel's meticulously researched accounts of Nakae's family, family friends, and his accidental connection with China are compelling and often moving. Among the family friends were Tōyama Mitsuru, a right-wing pan-Asianist, and Kōtoku Shūsui, an anarchist.

Nakae Ushikichi's father was the famous Nakae Chōmin, who translated Jean-Jacques Rousseau's *Du contrat social* and was a member of the first diet. Turning his back on the normal career promised to a man with his background, Nakae chose to drift off to China on graduation from the Faculty of Law of Tokyo University in 1914. The research department of the South Manchurian Railway Company in Peking was the most likely employer of young intellectuals who crossed the sea to escape from conventional careers as Nakae did or were squeezed out of the tight job market in Japan. Although Nakae stayed with the company only for a short while, there he met his closest friend, Suzue Gen'ichi, who devoted his life to the study of labor movements in Republican China. Nakae also worked as a live-in secretary for Ariga Nagao, a law professor and advisor to President Yuan Shih-k'ai. After a brief repatriation, he returned to China at the end of 1915 and witnessed the patriotic student movement ignited by Japan's Twenty-one Demands. Fogel highlights Nakae's courageous act on May 4, 1919, to rescue Ts'ao Ju-lin and his associates from the violent attack by demonstrators. Thereafter, under the generous patronage of this politician, Nakae lived a reclusive life with his wife, a former geisha at a Japanese restaurant in Peking.

A scholar without any academic affiliation, Nakae did not publish his writings except for one essay. Nevertheless, Fogel argues that Nakae's innovative methodology for the study of Chinese history was

important. In particular, his ideas about rural China inspired the influential theory of Shimizu Morimitsu that, in turn, sparked the long-lasting debate among Japanese Sinologists on the nature of rural community and despotism in China. The greatest portion of the book is devoted to an exposition of Nakae's thought and his study of the Chinese classics, *Shang-shu* and the *Kung-yang Commentary* on *Ch'un-ch'iu*. The author also peruses Nakae's notes on the works of Georg Jellinek, Liudvig Ignat'evich Mad'iar, Kant, Hegel, and Marx from which Nakae gathered food for thought. Fogel combed through unpublished and posthumously published writings of Nakae with loving care for this private scholar whom he idolizes as "one of Japan's most original Sinologists and thinkers of the Taishō and Shōwa eras" (p. 25).

Fogel portrays Nakae as a most humane and admirable man who developed a profound philosophy of life and underscores Nakae's warning against the Japanese invasion of China. Thanks to the author's painstaking research and empathic exposition, we now have a credible profile of this famous but little understood man. Yet we are still left with the vexing question of the political roles of prewar Japan's Sinologists such as Nakae. Despite his warning against Japan's military action, Nakae basked in intimate association with Japanese generals in China. This book does not critically examine the nature of Nakae's humanism and pacifism.

This is another major work by Fogel, who is deeply interested in Japanese Sinologists fascinated by China. His *Politics and Sinology: The Case of Naitō Konan (1866–1934)* (1984) has been translated into Japanese.

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WILLIAM MILES FLETCHER III. *The Japanese Business Community and National Trade Policy, 1920–1941*. Chapel Hill: University of North Carolina Press. 1989. Pp. x, 226. \$34.95.

This is a pioneering study of the role of big business in the formulation of Japanese national policy, particularly trade policy, during the interwar period. It has many of the virtues, and a few of the shortcomings, of any pathbreaking work.

William Miles Fletcher III considers two themes. One follows the rise of cooperation between the Japanese business community and the government during the interwar years. In so doing, he must first trace the formation of the organization of that business community, most notably through the Japan Economic Federation. The second theme focuses on the making of Japan's trade policies during these two decades of intense international competition or, as Fletcher rightly calls them, trade wars.

Readers are in for several surprises. Fletcher makes



clear that the primary drive for business-government cooperation came from business and that Japanese business felt compelled to organize itself (often in cartel-like arrangements) in response to foreign pressures. Business leaders frequently cited the American Edge Act of 1919 as proof that the Japanese government was lagging in its assistance to export industries. Trade crises, as with Britain over India in 1933, contributed great momentum to the "rationalization" of Japanese industries during that decade. Ironically, Japan's major trading partners have been at least partly responsible for the creation of "Japan, Inc."

The same rationalization of Japanese industry, however, that aided business-government cooperation and adjustment to the buffetings of foreign trade also rendered the business community vulnerable to the possibility of government control. Fletcher ably recounts the dogged and successful campaign of business leaders against Prime Minister Konoe Fumimaro's drive for a "New Economic Order" in 1940. Indeed, they coopted Konoe's drive for reform in their own unsuccessful attempt to create a ministry of trade.

The business community succeeded but only during the American Occupation. Fletcher narrates in discouraging detail the businessmen's eager collaboration with the Japanese military in creating a more ordered economic structure within Japan and in the blatant exploitation of the conquered territories. And the captains of industry and commerce do not score well in anticipating that Japan's foreign policies during the 1930s were bound to conflict with their own drive for increased trade with all the world.

Fletcher has deliberately cast a wide net over many industries and over two critical decades. His narrative necessarily raises many unanswered questions. Why was the textile industry so astonishingly well organized within itself? When and how did direct penetration of government positions by business leaders begin? What impact did the assassinations of Inoue Junnosuke and Dan Takuma in 1932 have on relations with the Imperial Army? What relationship did the business federations have with the political parties and the Diet, and why did they (apparently) impassively accept the parties' diminished influence late in the 1930s? As Fletcher notes, the rise and role of the business community in modern Japan is perhaps the least well studied aspect of an understudied nation's past. One hopes that his welcome contribution will spur much-needed additional work.

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TAKASHI SHIRAISHI. *An Age in Motion: Popular Radicalism in Java, 1912–1926*. (Asia East by South.) Ithaca: Cornell University Press. 1990. Pp. xxi, 365. \$34.95.

This book marks the end of one period in modern Indonesian historiography and the beginning of another. Its greatest strength is the judicious and splendidly detailed manner in which it shows that the previously customary methods of carving up and serving the history of the early Indonesian nationalism movement are inadequate. Scholarship has been moving very cautiously in this direction for some time, but Takashi Shiraishi makes the leap: the *pergerakan* (literally, movement) cannot be understood in familiar Western political terms (left and right, parties rather than individuals, ideologies rather than home-grown assemblages of ideas) and is not coterminous with the concept of nationalism. No future study can ignore the new look that this book gives the pre-1926 period or its implications for future scholarship.

Also of great importance is the book's convincing reinterpretation of the history and meaning of Sarekat Islam (SI), Java's first modern radical movement. Long misunderstood and misused by Indonesian and foreign historians alike, SI in Shiraishi's hands is far more complex and far more realistically portrayed than ever before. Neither a millennial movement nor a modern political party, it is shorn of its mystery with the help of old-fashioned historical detective work, some of it in rare periodical sources, and attention to social and mental context. The approach will be a model for a long time to come.

It is formulaic although no less accurate to say that, like any good revisionist study, this one raises more questions than it can adequately answer; the difference from less satisfactory works is, of course, that these questions are new and important. One example stems from the way Shiraishi brings down the curtain on the *pergerakan* in 1926–27. Something certainly ended with the famous revolts in those years, but was it the *pergerakan* in the larger sense? Contemporaries continued to refer to their movement as the *pergerakan* despite the changes with which they were, after all, familiar. Should not the reality of that continuity be recognized, explained, and to some degree acceded to, rather than dismissed altogether? Another question is raised with the notion that the *pergerakan* represented nothing so much as the translation of concepts, the moving of the world by words. Surely there is truth in the insight, which owes a good deal to poststructuralist thinking, but it remains as much a repeated contention as anything else. For we have here virtually exclusively the texts and ideas of the users of words and little of how their consumers interpreted and acted on them except, we are told, that they followed and moved. But what did it all really mean to those on the receiving end? Is it even possible to know?

Finally, Shiraishi begins by saying that the historiographical framework in which early Indonesian political activity is commonly placed was imposed initially by Dutch observers but then, in turn, by Indonesian intellectuals of a later generation, in



whose hands it became part of nationalist canon. This welcome point begs for expansion. Yet the author seems to end by saying that the classifications of nationalist, communist, and Islamic parties, the theories of mass party versus cadre party, and "the gloss of the *pergerakan* as an Indonesian national awakening" (p. 342) originated not among next-generation intelligentsia (and not with the Dutch) but within the *pergerakan* itself. Does this mean that the older generation and its ideas were the source of their own "misinterpretation" and that it was itself the origin of a natural process of change, and of changing perspective, which now we wish to correct because it is "misleading"? If so, the historiographical dilemmas thus raised require much additional discussion.

Fortunately, this book gives us an excellent and perhaps even indispensable place to start.

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SANJAY SUBRAHMANYAM. *The Political Economy of Commerce: Southern India, 1500–1650*. (Cambridge South Asian Studies.) New York: Cambridge University Press. 1990. Pp. x, 401. \$54.50.

This is an important and challenging study of the interlocking spheres of trade and politics in early modern South India. Relying on research in Portuguese, Dutch, and English archival and printed sources, Sanjay Subrahmanyam makes sharply angled revisionist arguments to challenge accepted interpretations. For example, he questions the recent benign view of an "age of partnership" between northern European and Asian states. Rather than an "age of partnership," this period was an age of "contained conflict" between the trading companies and Indian states (p. 7). Blockades, bombardments, ship seizures, massacres, and abductions were regular options for the European traders operating with unchallengeable maritime power in the seventeenth century. Little distinguished the calculated brutality of the Portuguese and the supposedly more pacific trading of the Dutch and English.

In discussing overseas trade, which he sees as tightly linked to both coastal and overland commerce, Subrahmanyam argues that remarkable growth occurred in the second phase of the Portuguese entry, after 1570. In the west, Cochin rapidly gained ascendancy as a result of Portuguese patronage. On the eastern seaboard, Subrahmanyam points to the strength of Portuguese private traders operating from San Thome to Melaka under loose royal concessions. The rise of Masulipatnam, Golconda's chief port, and its tie to Aceh in an essentially anti-Portuguese network were also part of this expansion. After 1600, three northern European trading companies—Dutch, English, and Danish—further drove this trade from Masulipatam, Nagapattinam, and Pulicat.

Subrahmanyam questions the suggestions by Michael Pearson and K. N. Chaudhuri that merchants and overseas trade were of little interest to the land-oriented rulers and military elites of the Deccan and South India. South Indian society was not a "hermetically sealed and cellular social structure" (p. 340) in which traders and ruling elites were rigidly segregated. Instead, hybrid traders and administrators, "portfolio capitalists," occupied "the middle ground between the worlds of mercantile capitalism and political capitalism" (p. 298). The activities of Achyutappa Chetti, the Telegu Balija Chetti merchant, revenue farmer, ship owner, and broker at Pulicat, or Mir Kamaluddin, the Persian port governor and ship owner in Golconda, demonstrate that the spheres of domestic and overseas commerce and politics were permeable in seventeenth-century South India.

Men such as these were heavily involved in revenue farming and offered their well-honed fiscal skills to their rulers in that capacity. Subrahmanyam, following Andre Wink, argues that revenue farmers were not oppressive tyrants wringing the last coin from the peasantry but traders with a rooted interest in the productivity of the rural population. This argument meshes with his view that a growing economic capacity in South India responded far more than previously assumed to indigenous agricultural and demographic expansion and much less to European export-driven demand.

For South India before 1650, Subrahmanyam's powerful insights are persuasive. After 1650 the intrusion of the Mughal empire and the Dutch and English East India Companies—large scale, highly centralized structures—brought change. As he admits, the European trading companies began to squeeze out independent Indian overseas ship owners and large-scale traders. In the Mughal sphere, rulers and nobles freely employed their vast personal wealth in overseas trading ventures. Trade was open to and of interest to the powerful. But the rise of "portfolio capitalists" involved in trade and revenue collection was muted. Apart from the famed Mir Jumla, virtually no merchants were favored with high imperial rank. The Mughals made revenue farming anathema and absorbed risks to gain direct control over officials. Revenue farming's reappearance in the post-1720 period was indeed a sign of lost imperial control. During periods of political flux and economic expansion, "portfolio capitalists" may have flourished; during periods of centralized power, their spheres were more limited.

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J. R. I. COLE. *Roots of North Indian Shī'ism in Iran and Iraq: Religion and State in Awadh, 1722–1859*. (Comparative Studies on Muslim Societies, number 6.)

Berkeley and Los Angeles: University of California Press. 1988. Pp. xiv, 327. \$38.00.

Although Shi'ism has remained a minority religion within the multiconfessional environment of the Indian subcontinent, followed by no more than 3 percent of the Muslims (p. 91), its sociocultural influence has surpassed its size and geographical distribution. One reason for the success of Shi'ism, at least from the early eighteenth century, was the growing size of the Persian diaspora after the catastrophic collapse of the Safavid empire (1722) and the ensuing "brain drain" that continued, as J. R. I. Cole ably demonstrates, for more than a century. The kingdom of Awadh (now part of Uttar Pradesh), which was created by a dynasty of Persian origin on the remnants of the Mughal empire not only adopted the Shi'i creed but possessed the necessary means to employ the Shi'i "men of learning."

In his book, a brilliant work of scholarship and historical insight, Cole studies the tripartite interaction between the Shi'i clerical establishment (in particular the Usuli creed dominated by Persian jurisconsults), the Awadh monarchy, and a religiously diverse society. He asks "whether one form of preindustrial religious organization is more likely to produce modern communalism than another" and argues that "in North India the process of community formation [was] promoted by the Shi'i learned men (ulama) and notables" (p. 2). Employing an impressive array of Persian, Arabic, and Urdu sources as well as archival and other European material, Cole combines social history and historical sociology of religion to produce an account that surpasses all available studies in its historical breath and analytical depth.

In part 1, Cole gives a succinct account of the evolution of Shi'ism in Iran, Iraq, and North India as a historical background to his excellent review of the state formation and the role of the Shi'i ulama in Awadh. In part 2, Cole looks at the origins and development of popular Shi'ism in the kingdom. After giving a thorough picture of the social composition of the Awadh society, he then draws on historical anthropology to examine the significance of state patronage for the spread of the Shi'i communal rites and in particular the observation of Muharram. He convincingly demonstrates how the presence of various modes of popular and mystical loyalties among the notables, the artisans, and the lower urban groups was employed by the state to create a broader popular base. In part 3, the focus of the study, Cole argues that the growth of the Shi'i clerical community (for which he employs the term "hierarchy") led to greater socioeconomic independence for the jurisconsults. As part of a pattern of Usuli prominence in Iran and in Shi'i Iraq at the turn of the nineteenth century, the Awadh ulama succeeded in augmenting their social status first by doctrinal (or, as Cole calls it, "ideological") shifts, most significantly the mandatory observation of the Friday congregational prayer, which was

highly symbolic of their dual intention to sanctify the state and to assume the religious leadership of the community. Blessed by the state's patronage, they dominated the theological colleges and religious courts and confronted their doctrinal opponents inside and outside the Shi'i camp. Securing the management of the alms and religious dues as well as the notables' largess—among them the famous Awadh Bequest—they achieved a pragmatic understanding with the state, which though not free from occasional tensions, was stable enough to fit the ancient Perso-Islamic model of the state-clergy alliance.

Not surprisingly, as Cole skillfully shows in part 4 of his book, the jurisconsults of Awadh were more unanimous and vocal than their counterparts in Qajar Iran in acknowledging their rulers' legitimacy, a factor that nevertheless was only partially effective in keeping the kingdom viable and its heterogeneous populace subordinate. The case of the Faizabad temple, a source of Muslim-Hindu sectarian hostility, should be recognized, perhaps with greater emphasis than Cole permits, as a sign of the failure of the Awadh rulers to arrive at a viable social equilibrium conducive to their political endurance. The Shi'i establishment, so lavishly nurtured by the nawabs, thus proved to be a source of social polarization and internal strife. Moreover, facing the British threat of annexation, it showed no serious political awareness, let alone acted as a nucleus for popular resistance. In this respect the Shi'is were far less defiant than militant Sunni clerics such as Barelavi. Orphaned by the loss of royal patrons, the Shi'i ulama "formed just a minority in northern India with their traditional privileges and forms of wealth open to being whittled away by the British and by ascendent Sunni and Hindus" (p. 282).

It may thus be argued, as a logical conclusion to Cole's emphasis on "communal enclosure," that the state-sponsored Shi'ism of Awadh was too preoccupied with its own vested interests to be able, or willing, to rally a popular movement in support of the dying kingdom, a fact that may testify to the absence of any inherent characteristics, such as political militancy, within Shi'ism, even when confronted with communal rivalry or imperial threat.

Cole's exacting scholarship and perceptive analysis have produced a masterful work of lasting significance.

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NITA KUMAR. *The Artisans of Banaras: Popular Culture and Identity, 1880–1986*. Princeton: Princeton University Press. 1988. Pp. xix, 279. \$29.50.

Finley Peter Dunne's Mr. Dooley once complained to his friend Hennessey that history usually taught us what people died of while he wished to learn what

they lived of. Nita Kumar's first book would suit Mr. Dooley's taste, for it is focused precisely on the life and leisure of the artisans resident in the north Indian city of Banaras. It sustains a rich descriptive portrayal of collective behavior that constitutes what might be termed the "popular culture" of these Banarasis. This is no easy task because the author has both to introduce and translate an entire constellation of indigenous ideas and practices rooted in the prosaic "realities" of "Banarasi-ness" and to offer a richly textured theoretical analytical framework that aims at uncovering "the 'structure' in 'history' and the 'history' in the 'structure'" (p. 8).

Banaras makes an inherently interesting subject for study. Its role in the Hindu world as perhaps the most sacred site on the sacred Ganges, the goal of myriad pilgrims, is well known. It is also a vibrant commercial and artisanal center, however, and Kumar demonstrates vividly that, to the citizens of Banaras, what is special about the place is not that theirs is a city of ultimate release (*moksha*) but that it is a place they enjoy. Through history and ethnography Kumar develops an indigenous view of time as "appropriate-ness." She carefully documents popular activities and sensibilities that evoke untranslatable moods of good feeling, freedom, carefreeness, and absorption in acts as ends in themselves—an identity of *Banarasipan* (Banaras-ness) shared by all artisans. Indeed, she argues that an "ideology of leisure" exists among artisans in which the freedom to control their time is an important element of their identity and survival. Furthermore, she suggests that their collective identities are shaped more by occupation and neighborhood than by religious ideologies or affiliations.

Change is important here. Kumar's survey of festivals, performances, and institutions demonstrates significant innovations over the past hundred years that contradict the conventional wisdom shared by both outside scholars and Banarasis themselves about the nature of tradition. Within her ethnographic field work, she uncovered evidence of a shift in traditional leisure activities and attitudes from being enjoyed universally toward being left almost exclusively within the domain of the artisans and laborers, "the transformation of an all-Banarasi culture into a lower class culture" (p. 110).

Within the limited space allocated for this review, it is impossible to convey fully a sense of the richness of Kumar's documentation. Much of her historical insight derives from local police station records—a vital resource to which few scholars ever hope to gain access. Some readers may wish that Kumar had drawn out with more detail her conceptions of communal identity and ideology. I feel that the author's discussion of "popular" and "textual" beliefs and practices of Hindus is more complete than her analysis of those of Muslims, although this may reflect her view that these communal identities are less significant.

There creeps into her account the voices of the

informants who seem to recognize, and regret, the trends of change, which evokes a mood of deterioration and decay. Perhaps this is the result of education, capitalism, population growth, urban expansion, or "modern times," but certainly north Indians have long indulged in a nostalgia for the "good old times now lost." The *shar-i-ashob* poetry of eighteenth-century Urdu bewailed change at a time when many of the traditions of Kumar's Banaras scarcely had been born.

This engaging and important work deserves an audience beyond those historians and anthropologists who study South Asia. The author situates her work in the intellectual arena of comparative social history. If the concerns of comparative study tend to be overwhelmed by the complex and vivid representation of Banaras and its citizens, this rests in the nature of the subject that Kumar has handled with such sensitivity and wit.

FRANK F. CONLON  
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NORMA EVENSON. *The Indian Metropolis: A View toward the West*. New Haven: Yale University Press. 1989. Pp. ix, 294; 258 plates. \$50.00.

No more conspicuous monuments to the long British presence in India remain than its great cities. Yet, except for New Delhi, they are remarkably lacking in monumentality. If Rome made its colonial outposts express the might and virtue of the empire, the British rulers and their Indian subjects were equally unconcerned about the outward appearance of the cities they built and occupied. In a thoughtful and informative study of Madras, Calcutta, Bombay, and Delhi, Norma Evenson shows how, in the absence of any consistent or shared vision of urbanity, they achieved their present shape and condition.

Monumentality was at best a sporadic element in the cities of Britain itself, and its overseas governors were as averse to architectural and urban extravagance as the vestries and town councils at home. The Indian urban middle class was as reluctant to support municipal improvements as English ratepayers. The great Indian cities therefore grew as products of market and demographic forces, not as reflections of imperial policy.

Madras has been the least self-consciously urban: a mixture of once-rural villages and modified versions of British villa suburbia, it became "an urbanized region based on low-density housing, with scattered commercial and institutional facilities." Never much industrialized, it is the poorest of the four, yet "an agreeable backwater" (p. 12).

Calcutta, the former capital, was as sprawling as Madras but more congested. For all of its great wealth and magnificent public buildings, it seemed to its British and Indian inhabitants, alike preoccupied

with making money and returning home. "a place of temporary residence" (p. 23).

The narrow peninsular site of Bombay gave it a more compact structure than either Madras or Calcutta and encouraged high-rise development. It also experienced a higher degree of geographical, social, and cultural mixture within its varied and cosmopolitan population. Less culturally sophisticated than Calcutta, it has enjoyed the greatest economic success of any Indian metropolis.

A sense of the transient and provisional characterizes all Indian cities, with the ironical exception of Sir Edwin Lutyens's and Herbert Baker's New Delhi, laid out at the very end of the empire whose permanence it proclaimed. Its classical public buildings, set amid wide ceremonial avenues and open spaces, contrast sharply with the antique splendor and modern squalor of the adjacent Mogul capital.

Architecture, typically designed by engineers, has over the past three centuries consisted of adaptations of European forms to local conditions. The long reign of classicism was succeeded, toward the late Victorian period, first by a succession of stylistic revivals, then by attempts to employ Indian decorative motifs. The buildings that resulted might have been expected to display either stereotyped mediocrity or tasteless eclecticism, yet Evenson's illustrations convey a continuing sense of energy, ingenuity, and vitality. Her last two chapters deal with urban planning and architecture since independence.

One can well understand why "the contemporary Indian metropolis both enthralls and dismays" Evenson (p. viii). Combining visual delight with structural disorder, economic achievement with social misery, cultural vitality with sanitary horror, the cities would seem to defy coherent analysis. Yet Evenson has lent clarity and order to their variety and contradiction, while throwing oblique light on the intruding Western urban and architectural tradition.

DONALD J. OLSEN  
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JUDITH M. BROWN. *Gandhi: Prisoner of Hope*. New Haven: Yale University Press. 1989. Pp. xii, 440. \$29.95.

Judith M. Brown has written an eloquent and faithful account of Mahatma Gandhi's struggle to forge a unified, self-respecting nation out of the heterogeneous and intimidated populace of British India. This work differs from her previous books on Gandhi's political career in the space she has allotted to straightforward, sensible discussions of his religious development, social philosophy, and private life. The result is perhaps the most successful attempt thus far to integrate the inner, religious man with the outer, political man.

Brown's Gandhi is a very real human being. What made him different from most men was his insistence

on living out his philosophy with no false divisions between thought and action, religion and politics, or his private and public lives. He extended to India itself this concept of oneness, derived from Advaita philosophy, and his major passion was to erase the false divisions among India's castes, communities, religious groups, social classes, and provincial nationalities. For the purpose of unifying his people, he tried to remake the Indian National Congress into a quasi-religious organization. He maintained that it represented all Indians, and from its leadership he demanded unselfish, full-time, devoted service to the poor of India. Although his ideal was beyond the capacity of most Congress leaders, his influence lived on in his chosen successor, Jawaharlal Nehru.

Brown's portrayal differs from the Gandhi made familiar by other biographers. First, her emphasis is on Gandhi in the prime of his life—a thoughtful, rational, political strategist, well educated and well read, an able organizer and fund raiser, who racked his brains for new ideas, particularly ideas on how best to use Satyagraha, his favorite weapon. He was also a person frequently ill and in physical pain, moody and depressed, who worked himself frenetically to the point of exhaustion, plagued by doubts and tormented by the failure of himself and his followers to live up to his extraordinary ideals of self-sacrifice and nonviolence. As her title indicates, he was also "a prisoner of hope," ever optimistic about the human capacity for unselfish love and for living in harmony with others.

With Gandhi's critics in mind, Brown reminds us that his inconsistent and often-incomprehensible political behavior can only be understood as that of a person on a religious quest whose fundamental purpose was to see God face to face. He tried to base his action on the highest ideals, yet by acting, as he knew he must, he involved himself in the world of compromises, deals, strategies, and power maneuvers. His goal was not political success but the improvement of his fellow human beings, and the core of his work was his social program among the peasantry—the promotion of cleanliness, good diet, basic education, communal harmony, economic self-reliance, and nonviolence. Brown's final assessment of Satyagraha is that it was less effective as a political weapon than as a self-strengthening and moral exercise. Gandhi blamed communal violence, the partition of British India, and the subsequent massacres largely on his own personal failures. Brown writes movingly of his final years when, as a lone Satyagrahi, he tried to restore calm in the most troubled regions of India.

The book has its flaws and problems, among them the large number of distracting typographical errors. One substantive problem is a lack of illustrative material. Brown's references to a political culture based on local interest groups and power brokers and her assertion that local politics drove the national movement are difficult to grasp without some examples. Similarly, Brown makes use of little of the rich



anecdotal material about Gandhi, for which abstract description is no substitute. One could also question her unqualified assertion that the India of today shows little or no influence of Gandhi and, in fact, has turned out to be the opposite of his dream. These, however, are minor criticisms of a book of universal interest, capable of touching the heart and mind of any reader regardless of field.

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#### UNITED STATES

PETER N. STEARNS. *Jealousy: The Evolution of an Emotion in American History*. (The American Social Experience Series, number 14.) New York: New York University Press. 1989. Pp. xiv, 225. \$30.00.

Relying on advice manuals, popular literature, court records, and opinion polls, Peter N. Stearns traces the history of American middle-class attitudes toward jealousy from the nineteenth century to the present. Stearns connects the shifts in jealousy standards to the wider history of emotions that he and Carol Z. Stearns laid out in *Anger: The Struggle for Emotional Control in America's History* (1986). If "jealousy was inhibited by strong community controls" in the eighteenth century, the weakening of those controls in the nineteenth century paradoxically resulted in "a more consistent set of standards bent on disapproval" of jealousy (p. 176). As the family weakened in the early twentieth century, Stearns finds that jealousy, though restrained somewhat by the "emotionology" of self-control, emerged within the context of authorized pursuit of personal pleasure and increased freedom from communal responsibility. Nevertheless, an ambiguous attitude served to restrain and channel jealousy into socially sanctioned behaviors. Fed by a decline in family-centered controls, jealousy developed into one of the central emotional concerns of the mid-twentieth century. The crusade to manage this epidemic of jealousy, Stearns writes, focused first and foremost on sibling jealousy, informed by the belief that its management would have a controlling influence on adult behaviors. Although this campaign produced complex results, it helps Stearns explain why "increasing provocations to jealousy" from 1920 to the present "were not matched by corresponding increases in jealous outbursts" (p. 114).

Beyond illuminating a long-neglected topic, Stearns's history suggests the need to modify the view that "emotions were substantially redefined in the eighteenth century" (p. 178). Rather, he offers the novel proposition that the nineteenth century was a unique stage in the history of the middle-class family. Additionally, this study challenges those psychological theories that have insisted that guilt historically supersedes suppressive behaviors.

As in all pioneering attempts, problems remain.

Stearns distinguishes here (as he did in *Anger*) between what he calls "emotionology" (the *mentalité* of emotions) and actual behavior or experience. Although the distinction is useful, it is not always clear where or when Stearns draws the line. Beyond this is the problem of definition. Jealousy is contrasted with envy as "the emotion attached to holding onto something or someone, involving fear of loss and anger or grief at its prospect; it might even serve, in social terms, as the emotion of the upper classes, who have more to lose" (p. 12). One need not quibble with this definition to notice its problematic nature. First, little consideration is given to the possibility that what his sources characterize as jealousy has itself evolved over time. Second, it is not evident that Stearns's sources always share his definition. This is particularly striking because the argument for the social control of jealousy in Victorian America rests almost entirely on the fact that it was rarely discussed: "That jealousy still went largely undiscussed by family-and-romance experts and ordinary people alike is one infuriating result, so that conclusions about jealousy's actual role and value have to be worked out somewhat indirectly or by speculation from the basis of a small if interesting body of explicit evidence" (p. 23). These issues aside, this book makes a unique contribution to the history of emotions.

HOWARD I. KUSHNER  
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STUART M. BLUMIN. *The Emergence of the Middle Class: Social Experience in the American City, 1760-1900*. (Interdisciplinary Perspectives on Modern History.) New York: Cambridge University Press. 1989. Pp. xiii, 434. Cloth \$49.50, paper \$14.95.

In his pioneering "Hypothesis of Middle-Class Formation," published in the April 1985 issue of the *American Historical Review*, Stuart M. Blumin proposed to uncover the particular experiences of "work, consumption, residential location, formal and informal voluntary association, family organization and strategy" (p. 312) that combined into a new middle-class "awareness" early in the nineteenth century. In response to the many studies of either working-class consciousness or urban elites in early industrialization, Blumin proposed to focus on the equally significant, albeit neglected, emerging middle class.

The results of his efforts are now available in a complex, searching book. Blumin wields a wealth of fascinating, if sometimes contradictory, evidence to prove that working with their heads as opposed to their hands was increasingly important for urban Americans. Blumin posits that the distinction between manual and nonmanual work, so familiar to twentieth-century Americans, is also critical in understanding the emergence of this middle-class self-awareness in the early nineteenth-century American city.



Blumin's most significant contribution is his extensive descriptions of pockets of antebellum Philadelphia: the increasing separation of nonmanual from manual functions in artisanal shops of the city center, the slow but unmistakable formation of white-collar residential neighborhoods, the distinct character of the middle-class home with its recognizable artifacts, and the new patterns of associations among like-minded white-collar souls. These sections, informed by a wealth of primary sources and a rich iconography, make this erudite book valuable.

Unfortunately, the study does not work as well as an explanation of how the middle class came into being. However intriguing, Blumin's theory of the early separation between manual and nonmanual work runs quickly into serious problems when put to the test of explaining class formation. By arguing that "the most significant boundary" in the late eighteenth-century urban society was the one that "separated the merchant and professional elite from everyone else" (p. 64), Blumin excludes from the middle class all "middling sorts" (skilled mechanics but also small entrepreneurs), because they remained below his manual-nonmanual "fault line," and treats the American "affluent upper stratum" as the equivalent of European aristocrats.

No matter how exclusive their parties, wealthy American merchants were not aristocrats and were singularly free from such aristocratic worries as degrees of nobility. Many became responsible for a political revolution that may not have immediately altered patterns of wealth, consumption, and social propriety but crossed Blumin's "fault line," paved the way for a social revolution, and contributed to the emergence of a middle-class ethos. Blumin has documented an important dimension of the class experience of Americans but by-passes the opportunity to think anew the connections between political and social change.

The expansion of commercial pursuits in a growing nation led to the growth of white-collar occupations in the nineteenth century. With a new white-collar world breeding additional white-collar occupations, every large city had, as Blumin puts it, "increasingly distinct centers of nonmanual work" (p. 86). Blumin is undoubtedly right in suggesting that this transformation of the work place played a major role in the self-recognition of the emerging middle class. But was the web of middle-class experience so directly dependent on the separation between manual and nonmanual endeavors? And was that separation as absolute and as frequent as Blumin describes it to be?

Blumin admits that he is looking only at the larger commercial cities of the nation (4 percent of the American population in 1800, 20 percent in 1900) with only occasional forays into smaller canal towns (p. 298). Even in the small commercial universe he studies, it is doubtful whether manual labor carried with it a "social stigma" (p. 107). Furthermore, the merchants' domination of the American economy was

increasingly challenged by a new type of manufacturer. That well-known story has major implications for Blumin's thesis. Even though industrialists hired an ever-increasing number of unskilled workers (hence the growth of the working class), it is hard to conceive of a manual-nonmanual "fault line" in an age when practically all industrial ventures (not to mention innovations) emerged from the shop culture of the tinkers and the small entrepreneurs.

A theory closer to the realities of nineteenth-century entrepreneurship might posit that, although a lifelong specialization away from manual work was the privilege of only a few, that restriction became the sign of a successful career long before it became the emblem of a class. Richard Hofstadter and C. Wright Mills were right in pointing out that a white-collar world large enough to affect what Blumin calls class "awareness" coalesced only in the late nineteenth and early twentieth centuries.

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MARGARET MARSH. *Suburban Lives*. New Brunswick, N.J.: Rutgers University Press. 1990. Pp. xvi, 231. Cloth \$32.00, paper \$13.00.

Margaret Marsh has added a fresh interpretation to the history of white middle-class suburbs in the United States. Her book takes a broad historical sweep from the early nineteenth through the mid-twentieth centuries and advances new ideas about the nature of suburban development. Marsh draws on prescriptive literature, housing designs, and promotional literature to examine changing social patterns and beliefs and uses examples of suburbs from each historical era to illustrate her points.

In contrast to scholars who have argued that suburbs have always reflected a domestic ideal, Marsh claims that suburban ideology has changed dramatically over time. She argues that, in the early decades of the nineteenth century, men and women had very different ideas about suburbia. Men favored suburban development as a means to perpetuate the Jeffersonian ideal of a democracy rooted in rural property ownership at a time when that political vision seemed threatened by urbanization. Women had to be convinced that suburbs would be inviting places to live; suburban boosters at the time believed that women would be reluctant to give up the conveniences and social life of the city. At the same time, middle-class women developed the ideology of domesticity, which empowered them within as well as outside the home but was not rooted in a vision of suburban life.

By the end of the nineteenth century, Marsh argues, the ideology of domesticity began to fuse to the suburban ideal, with a new emphasis on family togetherness. By that time, the suburb was seen not so much as the embodiment of the Jeffersonian ideal as

a means to preserve American democracy from the new threats of the modern city, particularly immigrants and racial minorities and the urban politics they influenced.

One of Marsh's most intriguing notions is that the Progressive era gave rise to what she terms "male domesticity," which emerged when middle-class men had time and energy to devote to family life. Marsh claims that male domesticity also served to attract women to the suburbs, because they could look forward to sharing child-rearing tasks more fully with their husbands. Male domesticity was premised neither on notions of equality nor on the belief that greater sharing of familial duties would free women to enter the labor force, although it did reflect a greater emphasis on "companionate marriage" and family togetherness. But male domesticity did not last. For reasons that Marsh does not fully explain, it had largely disappeared by the 1920s, and women were once again given full responsibility for family life and child rearing.

Marsh's identification of male domesticity in the early twentieth century raises new questions about the emergence of the "dad" in the 1950s. Dads were to serve, in part, as antidotes to "Momism": unless men paid attention to their children, the overbearing or overprotective attentions of their mothers would turn them into "sissies." If Marsh is correct that male domesticity flourished briefly among the white middle class in the Progressive era, its rise, fall, and reappearance in a new form after World War II calls for more study.

Marsh's most important contribution to our understanding of suburbia lies in her demonstration of the changing nature of the suburban ideal and its close ties to political transformations. Pioneered by middle-class and upper-middle-class reformers and developers, suburbia articulated the fears and aspirations of affluent white Americans as they continually encountered a changing nation. Marsh provides one more compelling study to remind us that private life is inextricably tied to public and national concerns.

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JON BUTLER. *Awash in a Sea of Faith: Christianizing the American People*. (Studies in Cultural History.) Cambridge: Harvard University Press. 1990. Pp. xii, 360. \$29.50.

We have come to expect the unexpected from Jon Butler, and this book is no disappointment. Gentle in disposition but iconoclastic in intellectual bent, Butler has challenged through the years the conventional interpretations of America's religious history. He has forced historians to examine their assumptions and, in doing so, has compelled us to admit distortions we have forced on the sources. Here Butler shapes his

research into a coherent alternative interpretation of the period from settlement through 1865. At the heart of his views are a critique of the overemphasis on Puritanism and his call for a model reflecting greater complexity and regional specificity. He finds both complexity and vitality in the religious situation by examining not only the works of the leaders but also the practices of the laity. Butler insists that early American religion must be understood in a European context and not in isolation. These judgments come together in his conclusion that religious developments after 1700 must be read as a tale of Christianization rather than as a story of religious decline.

Butler begins by describing the variety of religious beliefs and practices found in Europe where state churches stood side by side with a host of other forms of supernaturalism, including magic, divination, and astrology. During the first century of colonization, the state church tradition did not transfer intact to England's American colonies, and by the late 1680s much of the New World was bereft of traditional Christian influence. Even in New England, the churches were experiencing times of crisis. Yet magic and other forms of occultism continued among the population even though prohibited by law.

In the period from 1680 to 1760, according to Butler, a major religious revolution occurred. The European pattern of state churches was renewed in America, the landscape was sacralized, and even dissenters established elaborate denominational institutions. These same years witnessed the massive destruction of African religious traditions among the slaves—what Butler calls "an African spiritual holocaust" (p. 130), a move rationalized by the white clergy. Anglican and Congregational establishments also worked their will on many who were of a different mind.

Butler's account of early American religion removes New England from center stage and reintroduces a significant role for the Church of England. He demonstrates that the Anglican renaissance occurring in Virginia and elsewhere had significant implications for religious life throughout the colonies. Another effect of his argument is to reduce the impact of the eighteenth-century revivals, what he tagged several years ago the "so-called Great Awakening," and to underscore the pluralism within evangelicalism. He also believes that the role of evangelicalism within the American revolution has been overstated. By the time of the Civil War, America was a "spiritual hothouse" populated by a variety of religious traditions, many of which joined Christianity and popular supernaturalism. Among these were Methodism and Mormonism, Afro-American Christianity, and spiritualism. In the antebellum years there was also a shift from state coercion to the principle of volunteerism and a resulting increase in the ascendancy of the Christian churches.

Butler's volume is rich with detail and insight. It

will provide a whole generation of scholars with a new agenda.

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Bloomington

HARRY C. BOYTE. *CommonWealth: A Return to Citizen Politics*. New York: Free Press. 1989. Pp. xvii, 221. \$22.95.

"Politics among equal citizens and assumption of responsibility for public goods," Harry C. Boyte asserts in this passionate book, "makes up the commonwealth tradition in American political culture." This tradition, Boyte argues further, "forms an alternative to the politics of modern capitalism and socialism alike" (p. 13). After pointing out "commonwealth" emphases in the Puritan colonies and in the era of the American revolution and nation building, Boyte sees a particular flowering of the tradition in the turn-of-the-century Populist and Progressive movements. Tragically, though, during the twentieth century, this emphasis on "strong citizenship," oriented toward pursuit of the common good, gave way steadily to a view of public life where "experts, professionals, technocrats, and managers—purportedly far more qualified than the citizens at large—acquired growing authority as the vanguard of scientific decision making" (p. 36). This implicit disdain for the active citizen, Boyte argues, became part of the liberal corporate state, which from the time of the two Roosevelts came to dominate American reform movements as well as government at all levels. In decrying this tendency, and in finding the modern liberal mainstream bankrupt, Boyte joins a chorus of critics of contemporary American public life.

Both to reveal the shortcomings of "liberal" conflict-of-interest politics and to show that the commonwealth tradition survived even in Ronald Reagan's America, Boyte examines the ideas and practices of some modern grass-roots political action groups. The efforts of Saul Alinsky's Industrial Areas Foundation (IAF) to organize politically effective poor, inner city groups, Gerald Taylor's Baltimoreans United in Leadership Development (BUILD), and the work of Terry Pettus in vitalizing citizen politics in Seattle, Washington, are cited as examples of citizen empowerment that reverse the trend toward elite management so damaging to effective democracy. Boyte is a modern Thomas Jefferson in his conviction that only effective participation by public-spirited citizens, especially in local politics, can make democracy result in good government. This volume is thus a vigorous appeal, in the tradition of Jefferson and John Dewey, for meaningful self-government.

Boyte accepts so much, though, of the self-oriented, special interest model of politics that he compromises his own argument. He notes, for example, that Alinsky's IAF sees the private sphere as "the

more self-sacrificial and idealistic realm, while the public is the world of quid pro quo and self-interest" (p. 98). Later he observes that for BUILD, "public life is an open and dynamic process initiated by diverse self-interests and disciplined anger" (p. 129). But this notion confuses the ideas of public and private by supposing, in a way analogous to the individualistic, enlightened self-interest model that Boyte generally condemns, that self-interests can be mobilized and then counterpoised to achieve the common good. He thus gives up the essential point that proper citizenship in a democratic society requires some public-spiritedness, that is, some capacity, in exercising the public office of citizen, to rise above private, special interests to understand and seek a larger public interest. It is thus fatal to the public good, especially in a democratic society where the people are the ultimate decision makers, to rely on some kind of benign interaction of selfish interests. Boyte generally realizes this, but in trying too much to accommodate grass-roots preoccupations, he opens the door to the very orientations that he properly sees as dangerous. In fact, if he would welcome rather than condemn the Aristotelian "neorepublican" tradition (which he caricatures), he would have just the resource and perspective he needs to guard his commonwealth and citizen politics against the virus of self-interest. This tradition calls not for the dismissal or denial of "self-interests" (p. 131) but for their acknowledgment and then inclusion in or subordination to a wider perspective—almost the same thing, one suspects, as Boyte's call for grass-roots "anger . . . given discipline and form by skillful public action" (p. 133). With this minor caveat, we can welcome Boyte's book as a worthy contribution to the important movement to revitalize an essential, Jeffersonian tradition in our public life.

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RICHARD NELSON CURRENT. *Phi Beta Kappa in American Life: The First Two Hundred Years*. New York: Oxford University Press. 1990. Pp. xiii, 319. \$29.95.

Founded in 1776 as a fraternity and forensic club by five students at the College of William and Mary, Phi Beta Kappa evolved into a society to honor scholarship, without doubt the most prestigious of its kind in the United States. Drawing on archives of the society, Richard Nelson Current has written an engaging account of Phi Beta Kappa's first two centuries, set in the context of the changing functions of the nation's colleges and universities. Originally commissioned by the society, the book is far more than a celebration. Indeed, Current ran afoul of the Phi Beta Kappa history committee, which deemed his manuscript too full of internal squabbles and attacks on the society's reason for being and removed its imprimatur from the book. Current does ask hard questions about Phi

Beta Kappa: How valuable is the key to those who wear it? Did the society discriminate against blacks and women? Did it squander its potential power, often staying silent as educational issues were debated? He works just as hard, however, to show how much the key was coveted and to document the social activities of Phi Betes. An impressive work, this study shows the strain of serving two masters, the muse and the membership.

Current provides a fascinating account of the rise of Phi Beta Kappa in the contentious collegiate culture of the nineteenth century. To weaken faculty resistance at Brown University, for example, President Francis Wayland replaced the popular Federal Adelpi with Phi Beta Kappa. And when fraternities usurped the society's social functions on campuses throughout the country, Phi Beta Kappa focused exclusively on academic achievement. Always resilient, the society also eluded attacks on secret societies during the Freemason frenzy. If Current explains how Phi Beta Kappa endured, however, he does not explain why. Before the 1930s, grades were relatively unimportant. Why, then, did students treasure the key? A merit society in elitist institutions in a democratic nation, Phi Beta Kappa may well embody the contradictory values at the heart of American culture.

Current also raises but does not resolve the issue of the relative distinction of Phi Betes in later life. Studies in the early twentieth century used *Who's Who in America* to argue that members were three times more likely to succeed than nonmembers. Current cites critics who pointed out that *Who's Who* gave too much space to clergymen, educators, and social workers, but he does not assess the "value" of a Phi Beta Kappa key, either in the professions or in the marketplace. An investigation of this kind would be immensely valuable not only to students of equal opportunity but also to those convinced that universities help society "manage ambition" by encouraging the highest aspirations of only a few. Did this concept play a role, for example, in the decision in the late 1930s to reduce the percentage of graduating classes admitted to the society?

This book, then, is a serious, scholarly work that demonstrates how much can be learned about higher education through an examination of an honor society. Though not the last word on the subject, the volume ought to make all members of the society proud.

GLENN C. ALTSCHULER  
Cornell University

DONALD R. HOKE. *Ingenious Yankees: The Rise of the American System of Manufactures in the Private Sector*. New York: Columbia University Press. 1990. Pp. xii, 345. \$40.00.

Donald R. Hoke's book is a direct challenge and indeed a refutation of Donald Hounshell's much

admired work *From the American System to Mass Production, 1800–1932* (1984). Hoke argues that the American system of manufacturing, "characterized by the mass production of interchangeable parts on specialized machinery arranged in sequential operation" (p. 20), originated not in the public armories as Hounshell maintains but in the private sector. His argument is built on four case studies—wooden movement clock manufacturing, axe manufacturing, typewriter and watch manufacturing—spanning the entire nineteenth and the early twentieth centuries (1807 to 1914). The impetus behind the American system, Hoke contends, was bright, enthusiastic, and innovative mechanics (engineers) and profit-seeking entrepreneurs. He does not exclude totally the role of innovation and practice derived from the public armories, but he comes close to doing so, especially in the antebellum period.

The merits of the book are many. An enthusiastic young historian, Hoke is very much devoted to his scholarship and to his "long interest in old mechanical things" (p. 2), and his enthusiasm and devotion clearly manifest themselves in this work. He has traced the interaction between entrepreneur and inventor or mechanic in the four case studies. Both were essential in the origin and development of the American system. His argument is persuasive, if not totally convincing, largely because of his expertise in the use of "objects," which he employs to illustrate the written evidence about them. The most striking feature of this book is the abundance of illustrated "objects" ranging from the earliest working parts of the wooden clock in the early nineteenth century to the more specialized machinery used to mass produce the "Waltham Watch" a century later, from the crudest hand-printing machine of the 1850s to an almost modern typewriter of the early twentieth century.

This is an impressive work on all counts, which makes some of the observations that follow seem trifling and petty. But the book could have been considerably improved had the editors insisted on one more revision of this doctoral dissertation. Such a revision should have eliminated much of the "chatty" and often redundant discussion of the serious differences that Hoke has with his "close friend" David Hounshell, which characterizes the early pages. The prose, although clear, is often wooden, particularly in the chronological discussions of the development of the four products about which he writes. He has a strong tendency to make his points by listing rather than by developing his ideas in an integrated and coherent essay.

Furthermore, Hoke claims that he has written a book with full treatment of the essential roles of both the mechanics and the entrepreneurs. In truth his study is largely of the mechanic and his technological role in the invention, adjustment, and development of a product. The economic role of the entrepreneur is mainly given lip service, and the social and political context is almost totally omitted.



These concerns only make a good book one that could have been better. As it is, anyone interested in the history of American technology and the beginning of the American system of manufacturing would do well to read this work.

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MIRA WILKINS. *The History of Foreign Investment in the United States to 1914*. (Harvard Studies in Business History, number 41.) Cambridge: Harvard University Press. 1989. Pp. xxiii, 1055. \$65.00.

Mira Wilkins, a leading business historian, has turned her attention to the history of foreign investment in the United States from the colonial period to 1914. World War I serves as a sensible dividing line, since it marked the end of the United States' long record as a debtor nation; indeed, during 1861–1914 the country was the largest debtor nation in the world. In compiling the wealth of information presented, Wilkins decided to limit her research to "long-term, nonresident investment from abroad" (p. ix), meaning foreign portfolio investment and foreign "direct" investment, with the latter obviously connected to possible management or control of an American enterprise.

That the American economy has always been inextricably linked to foreign investors becomes clear in Wilkins's vast documentation. British mercantile houses and shipping firms competed with Dutch and French interests for an American connection even before 1776, and French, Spanish, and Dutch investors helped to finance the American revolution. With political independence, the United States became increasingly dependent economically on overseas capital, stimulating public discussion of a topic that would remain constant throughout subsequent American generations: how to reconcile the need for desirable foreign long-term direct and portfolio investment with an uneasy concomitant concern over non-American ownership and control of American shipping, banking, and land. Americans expressed more than a passing interest in this theme even as foreigners supplied the capital for Alexander Hamilton's funding of the federal and state debts in George Washington's administration and for Thomas Jefferson's purchase of the Louisiana Territory. Jefferson's Embargo Act of 1807, the Non-Intercourse Act of 1809, and the ensuing War of 1812 interfered with American trade and investment relationships overseas, but the rise of Great Britain as a major creditor nation after 1815 and the corresponding attractiveness of the United States continued to draw foreign investment to American shores.

Then, with an American government intent on retiring its public debt, foreign investors soon became enchanted with individual American states as they sought funds for canals, railroads, banking institutions, and land companies. This heavy foreign in-

volvement in public sector securities and the subsequent default on these debt obligations by a number of southern and midwestern states seemed only to dampen temporarily overseas enthusiasm for state bonds. Soon the American conflict with Mexico, revolutions and turmoil in Europe, the discovery of gold in California, the further expansion of the American railroad network, and the resumption of interest payments by the defaulting states precipitated new, irresistible investment opportunities for Europe's migrating capital, which had been halted temporarily by the Panic of 1857 and the outbreak of the Civil War in 1861. A new generation of European investors bought huge amounts of southern state bonds during Reconstruction, only to be confronted by defalcations by ten of them. Unlike the post-1837 state bond defalcations, when all but Mississippi eventually resolved their embarrassments, not one of the ten states ever repaid these debts of the 1870s.

Wilkins also pays welcome attention to other objects of foreign capital preceding the Panic of 1873. A large percentage of overseas investments went into American railroad securities, and gradually the railway system joined with American steamships, cables, and telegraph lines in linking the United States to a world economy and immigration movement that further accelerated foreign investment. The author's passages relating the close alliances between British and American and then German and American investment bankers represent an excellent survey of the available literature on the subject.

In her final section, Wilkins takes up the role of overseas investment in the United States by private sector categories for the period 1875–1914: railroads and land; precious metals; coal, iron, and steel; copper, oil, and other minerals; food, drink, tobacco, and grocery products; textiles, apparel, and leather goods; chemicals; and banking, commercial, and communication services. Interspersed throughout are American and foreign attitudes toward the enormous foreign stake in the United States' economic growth.

Wilkins often juxtaposes her factual findings with viewpoints that confirm and, in some cases, contradict earlier wisdom. She accepts the prevailing view that foreign portfolio investment was far greater than foreign direct investment during 1875–1914. But her research fails to support the generalizations of Cleona Lewis, Margaret Myers, Douglass C. North, and others that the importance of foreign investment in American capital formation steadily diminished after the Panic of 1837.

Wilkins discusses a vast array of American and foreign entrepreneurs and firms in her lengthy book, and scholars in the field of American business and economic history will profit from the fifty-nine-page index. The book also contains over three hundred pages of footnotes, and, because the author uses them as running commentaries on the works and con-



clusions of other scholars, they are extraordinarily helpful.

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TIMOTHY SILVER. *A New Face on the Countryside: Indians, Colonists, and Slaves in South Atlantic Forests, 1500–1800*. (Studies in Environment and History.) New York: Cambridge University Press. 1990. Pp. xii, 204. Cloth \$39.50, paper \$10.95.

Timothy Silver's book provides an environmental perspective on the early history of the South Atlantic region (the lower Susquehanna River to the Okefenokee Swamp, the Atlantic Ocean to the Appalachian Mountains). He draws on three streams of recent scholarship about early America—the social and economic history of the Chesapeake and Carolinas, European-Indian contact, and ecological change—as well as on accounts left by contemporaries. Working from these sources, Silver creates an imaginative synthesis that focuses on how the human and animal populations and southern landscape changed between the period before Indian-European contact and the end of the eighteenth century.

One theme of the work centers on the way in which European and African peoples replaced the Indian population, which was decimated by infectious diseases (smallpox, measles, influenza) brought by settlers from the Old World to the South Atlantic, and, in turn, how the growth of coastal settlements in the eighteenth century made malaria and yellow fever growing problems. Regarding animals of the South Atlantic, Silver explains how European and Indian hunting virtually eliminated some species (buffalo, elk) and greatly reduced others (deer, beaver), and he traces the way European species (livestock, cockroaches, the Hessian fly) affected the South Atlantic environment. It is in treating the deforestation of the South Atlantic, a “natural” process furthered by the Indians but largely the work of European settlers, that Silver provides the most thorough and original part of his study. He probes the way the sustained clearing of land and the commercial use of trees created new patterns of vegetation, altered wildlife populations, changed weather conditions, and brought on flooding and soil erosion. Here, as throughout, Silver's concern is not to isolate specific changes but to demonstrate the relationship between changes in the human and natural environment. Unfortunately, he can recover much less about how conscious human actors were of the changes of which they were a part. Moreover, for those familiar with the work of Alfred W. Crosby and William Cronon, the argument, if not the detail, will often be familiar, but in reconceptualizing the way in which we view the early South Atlantic, Silver has made a substantial contribution.

Silver should be credited both for acknowledging the assumptions he brings to environmental history and for explaining his sense of the “meaning of the past.” Central to his work is the belief that however different Indians and Europeans were, the more fundamental divide was between nature and culture. It is more important, he contends, to understand the interaction of nature and culture than to blame humans (or capitalism) for destroying nature; rather, the lesson Silver discovers in the history of the South Atlantic forests is that of the human capacity to adapt culturally to a changing environment. One can find very different readings of the meaning of the environmental past (for example, in the recent work of Carolyn Merchant, *Ecological Revolutions: Nature, Gender, and Science in New England* [1989]), and, as an environmental perspective becomes increasingly important in both teaching and research, one hopes historians will remain sensitive to questions of both meaning and substance.

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WENDELL H. OSWALT. *Bashful No Longer: An Alaskan Eskimo Ethnohistory, 1778–1988*. (The Civilization of the American Indian Series, number 199.) Norman: University of Oklahoma Press. 1990. Pp. xviii, 270. \$21.95.

This comprehensive study by the foremost authority on the Yupik-speaking Eskimos of the Kuskokwim River region of southwestern Alaska offers unusual findings and conclusions. Traditionally the Kuskokwim people valued restraint, modesty, and deference, traits for which they adopted the English word “bashful,” although “self-effacing” may be more appropriate. Western contacts were limited before World War II, and such agents of change as fur traders and missionaries had to make many adaptations to the environment, but the authoritarian and assertive means by which westerners sought to achieve their goals significantly diminished the vitality of the Eskimo culture.

From the first half of the twentieth century, that culture was increasingly disrupted as American gold prospectors and traders entered the Kuskokwim, and epidemics of influenza and measles, together with the endemic ravages of tuberculosis, beset natives in the fifty-six regional villages. After 1941, U.S. military personnel further accelerated western influences, many of them highly negative, and contributed to the growth of Bethel as the region's principal town, still known today for its alcohol and prostitution as the “Sin City” of Alaska. Then the Great Society programs of the 1960s led to the establishment of a variety of social service agencies in the area and brought many naive though dedicated young whites. Finally, the landmark Alaska Native Claims Settle-

ment Act of 1971 imposed a corporate organizational structure on the region's people and furthered the intrusion of western ideas and practices.

Wendell H. Oswalt concludes, however, that despite these western influences, such traditional patterns as a high reliance on subsistence hunting and fishing, intravillage sharing of resources, and the extension of hospitality without expectation of reciprocity remained viable. Above all, villagers sustained a strong attachment to the land. And when the institutions created by the claims settlement began seriously to erode the functioning of local autonomy and threatened the village land base, the Kuskokwim people reassessed their circumstances and became bashful no longer. They have expressed their concerns through the native sovereignty movement, which is designed to reassert the significance of the villages and their self-governing autonomy by wresting control of affairs and environment from outsiders, native and non-native. Starting in the upriver village of Akiachak, which revoked its status as a second-class city in 1983, the sovereignty movement now includes nearly half of the native population of Alaska and has led to the introduction of proposed legislation to guarantee local authority over the land and its people.

Oswalt rejects both dependency and chronological or topical eras as organizing principles for his study, preferring instead the concept of "access," defined as "the capacity of outsiders to reach and subsequently frequent a locality in which they seek to achieve particular goals" (p. xvi), an idea that fits neatly the objectives of the new movement that he describes. He finds that restoration of village sovereignty has halted continuously expanding access by westerners to the Kuskokwim communities and may present a new pattern for native-western interaction. The book is the most complete account of any native Alaskan group and is persuasive in its positive assessment of the burgeoning native sovereignty movement.

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CAROLYN MERCHANT. *Ecological Revolutions: Nature, Gender, and Science in New England*. Chapel Hill: University of North Carolina Press. 1989. Pp. xv, 379. Cloth \$34.95, paper \$13.95.

Readers who know Carolyn Merchant's first book, *The Death of Nature* (1983), will find this new study both familiar and surprising—familiar in its effort to connect ecology and feminism and surprising in its shift from Europe to America and from intellectual to social history. The book attempts to explain two "ecological revolutions": the seventeenth-century vanquishment of Algonquian agriculture by English husbandry and the early nineteenth-century transi-

tion in Anglo-American agriculture from subsistence to commercial farming.

Merchant has not entirely abandoned her earlier interests. A stimulating chapter entitled "The Animate Cosmos of the Colonial Farmer" discusses the agricultural theories of Aristotle, Pliny, Paracelsus, and a host of seventeenth-century European philosophers, alchemists, and agricultural improvers, connecting all of these theories to New England via Harvard textbooks, the writings of ministerial elites, almanacs, but, alas, only one short quotation from an actual farmer. This chapter however, is unusually cerebral. Most of the book literally stays close to the ground. Merchant has the gift of being able to make plain dirt interesting. Sections on such venerable topics as Indian use of fish fertilizer or upcountry girdling of trees are almost poetically concrete.

In the opening pages, she sets out a "conceptual framework for interpreting ecological revolutions." Human existence, she argues, is predicated on an ecological core from which production, reproduction, and consciousness extend. An accompanying diagram (p. 6) suggests the complexity of her model. The relationship between human societies and nature is never static. Changes in production can alter the ecological center just as reproduction, which for humans includes socialization and governance as well as procreation, interacts with and shapes production.

The chapters that follow are eclectic, even at some points eccentric, ranging from rich descriptions of objects, habitats, and processes to sometimes convincing, often tenuous, attempts to map long-term change. Merchant is clearly more comfortable contrasting fixed types—corn mothers versus Puritan fathers, subsistence farmers versus market agriculturists—than in delineating causal and chronological sequences. She implies (p. 86) that the English ships that brought rye, wheat, and plows to the New World also brought bubonic plague and smallpox. Actually, the pathogens, borne by transient fishermen and traders, came first, a fact with enormous consequences for New England history. Seeing the bones and abandoned fields of Indian villagers, Puritans had reason to believe that Providence had designed their settlement.

A similar casualness about causality makes it difficult to understand how the descendants of the witch-hunting Puritans whom Merchant describes in chapter 3 became the benign subsistence farmers of chapter 5. The descriptions of subsistence farming are more convincing. Here she confronts the recent scholarly debate over the market orientation (or lack thereof) of New England farmers. She argues that historians have given too much credence to formal account books and too little attention to casual barter and trade, to women's work, and to hunting and gathering. Her use of debt allotments to supplement evidence from probate inventories and tax lists is intriguing, as is her discussion (in an endnote) of her own effort to bake and compute the caloric content of

"rye 'n Injun" bread. The heart of her argument is in such details. Even without the preface and epilogue, a reader might discover in her passionate concern for soils, nutrients, and polycultures a belief that the "native American and colonial past we have lost in many ways enjoyed a more vital relationship with nature" (p. xiii).

Merchant also argues, as in her first book, that human disregard for the earth is connected to male domination of women. Puritan fathers "legitimated the subjugation of wilderness" through metaphors of virginity and marriage (p. 100), and nineteenth-century reformers urged farmers to struggle "with mother earth" and force her "to yield to you her hidden wealth" (p. 204). The argument is suggestive but incomplete. Commenting positively on the "environmental ethic" contained in Penobscot corn legends, Merchant ignores the gender issues raised by those same stories, in one of which a mythical woman asks her husband to "kill her with his ax, then drag her body through the forest clearing until all her flesh had been stripped" (p. 72). With Carolyn Merchant as a teacher, it is difficult to ignore such a text.

This book is an ambitious and courageous effort. Its failures are in part a mark of how little historians yet know about the intersection of race, gender, and agriculture in early New England.

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JOHN CANUP. *Out of the Wilderness: The Emergence of an American Identity in Colonial New England*. Middletown, Conn.: Wesleyan University Press. 1990. Pp. xiii, 303. \$35.00.

This book spotlights the reactions of New England settlers to their new environment and its native inhabitants from 1620 to the early 1700s. John Canup probes through and beyond the lines of early promotional literature to discern "a rhetoric of degeneration" (p. 10), intimating that a civilized people transplanted into a wilderness may revert to a bestial condition. Understandably, such concern was sustained by initial difficulties in taming the wilderness, but the colonists' perturbation increased when the effects of wild nature failed to recede after civilized conditions had been achieved. They perceived, for instance, special "wilderness connotations" in a rash of cases of animal buggery in the early 1640s (p. 43). Those cases revealed that "something [distinctive] in America" (p. 40) was at work, namely the power of nature to spur into action the human "wilderness within" (p. 33). Of such power, too, a shockingly full illustration was provided by the natives, seen by influential leaders as hopelessly degenerate descendants of once proud Old Testament peoples. Such concerns led the Puritans to a distinctively American envisagement of their culture as a never-secure bulwark against brutish wildness and intensified their

communalism. In these and similar ways, visions and fears of the wilderness played a surprisingly large role in the process of Americanization through the seventeenth century, but they also complicated it. For by feeding suspicion that degeneration was an inescapable effect of the American environment, they led the colonists to doubt their ability to maintain their Englishness and thus contributed to an ambivalence toward American identity and to the groveling provincialism and Anglophilia of the generation of Cotton Mather.

This book takes its place in the remarkably large and growing body of American scholarship on Puritanism defined by literary and American studies disciplines. Canup's special sensitivity to tensions and ironies in the colonials' "rhetoric" shares in one of the prominent ardors of contemporary scholarship and proves fruitful in a number of ways. From a relatively small fraction of the total body of early New England writing, he succeeds in teasing useful inferences and builds an interesting and largely cogent argument. Yet behind Canup's own rhetoric lies the vexingly common assumption that the term "Puritan" and its cognates have a self-evident, generally understood meaning and thus require no explanation. If earliest New England was a religiously distinct enterprise, its special theological enthusiasms must have imprinted deeply both the colonists' response to the wilderness and their experience of Americanization. Canup struggles with the Puritan dimension, but his evident skill in rhetorical criticism is not consistently complemented with a technically adequate command of the religious and intellectual history that shaped the colonists' rhetoric. Indeed, the argument of the first chapter begins with a probable mistake. John Cotton, readers are there reminded, admonished the departing settlers of 1629 that their settlement might "degenerate" (p. 10), but this standard deuteronomic warning is then construed as a forecast that American nature might prove hostile to English culture. Here is, indeed, a logical beginning for the argument, but how does it cohere with the text's plain connection of degeneracy to moral deviation within the external covenant?

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HAROLD E. SELESKY. *War and Society in Colonial Connecticut*. (Yale Historical Publications.) New Haven: Yale University Press. 1990. Pp. xvi, 278. \$23.50.

In this very capable contribution to the "new" military history, Harold E. Selesky analyzes the changing social and military implications of raising and maintaining a colonial fighting force. For most of the seventeenth century, Connecticut settlers fought what were (from their English point of view) defensive actions against the Dutch, the French, and, above all, Indian tribes. Militia units mobilized quickly and

effectively, and the settlers came out of the seventeenth century with a sense of self-confidence in their ability to defend themselves. In the eighteenth century, however, the colony became involved in England's imperial wars and, as a result, had to develop new methods to recruit men for military action on the New England frontier, in Canada, or even as far away as the West Indies. Not surprisingly, many settlers were reluctant to enlist, primarily because they did not feel an immediate threat close to home. Accordingly, the colonial government had to fill the ranks by impressing white militiamen or by relying on Indian allies. But as the demands for men continued, the government increasingly turned to another approach, attracting recruits with money. By the middle of the eighteenth century, Selesky writes, "the idea that soldiers were members of the community temporarily bearing arms in time of danger gave way to the assumption that men could be hired to perform dangerous and disagreeable work" (p. 154).

The relationship between self-sacrifice and self-interest is one of the main themes of the book. Connecticut's leaders committed their colony to imperial warfare not only because they felt a foreign threat or a sense of patriotism. They also wanted to curry favor with the crown so that they could both attract imperial revenues and avoid imperial interference in their internal affairs. Common men signed up to serve in Connecticut's regiments primarily for bounty money and regular pay; in the increasingly straitened and stratified economy of colonial Connecticut, military service seemed a hopeful source of monetary gain. No one got rich in the regiments, of course, but the system was still successful in one important respect: by the end of the colonial period, Connecticut had developed a quasi-professional fighting force, something between a militia and a standing army, and the experience and confidence Connecticut's soldiers gained in the imperial wars stood them in good stead when they joined the revolutionary movement to break away from the empire. But making that break meant losing the empire's money, and Connecticut's leaders soon found out that they could hardly keep an army motivated by *rage militaire* alone. "Had the leaders realized that a war of independence would make revolutionary demands on the resources of their society," Selesky concludes, "they might have thought twice about an appeal to arms" (p. 243).

For the most part, this is a book written from the perspective of those leaders, and it offers a better analysis of policy than an appreciation of people. Although Selesky includes some brief discussions of camp life and combat, he seldom explores the wartime world of the common man. He depicts Connecticut's soldiers as brave but barely competent, more successful at fortifying than fighting. Their Indian allies (who comprised as much as a quarter of some units) are almost always dismissed as drunks. That may be the top-down view found in the official

papers, but one wonders if a more skeptical and sensitive reading of the sources might not have produced a more fully textured understanding of the social experience of soldiering. In general, Selesky has written a book that is quite convincing as political history but not altogether compelling as social history.

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MARY M. SCHWEITZER. *Custom and Contract: Household, Government, and the Economy in Colonial Pennsylvania*. New York: Columbia University Press. 1987. Pp. xii, 271. \$32.00.

Studies of southeastern Pennsylvania in the colonial era are not yet in the same league—for number and sophistication—as works on the New England town or the Chesapeake. But the region is definitely a comer. Mary M. Schweitzer examines Penn's Colony between 1720 and 1755, with much of the detailed analysis drawn from Chester County materials. Roughly a third of the book looks at households, while the balance takes a broader view of the economy. Her work enriches our knowledge of colonial Pennsylvania, although several of its key arguments may be questioned.

Schweitzer's findings about the household economy expand and confirm recent studies. She details the diversity of activities—agricultural and artisanal—in Chester County, echoing the conclusions in James Lemon's *Best Poor Man's Country* (1972). Her discussion of a labor market that included indentured servants, free workers, and slaves is useful if familiar. She also shows, as have others, that a family's wealth reflected its stage in the life cycle. Couples just starting out owned less property than those farming for many years. Her emphasis on the farm family and its local orientation places her on the side of historians such as James Henretta and Michael Merrill (rather than, say, Winifred Rothenberg) in the debate over whether early Americans were community-oriented agriculturalists or budding capitalists.

Most of the work views the Pennsylvania economy as a whole, and it is here that Schweitzer's findings are more original—and more controversial. She argues that "economic growth in Pennsylvania in the 1700s was strongly affected by economic policies chosen by the colonists themselves" (p. 217). She singles out three legislative initiatives—all adopted in the early 1720s—as spurs to development. The three are the introduction of paper money, the creation of a loan office, and flour inspection.

We can examine this argument more closely. Her contention that the economy achieved "substantial growth" (p. 84) between 1717–21 and 1748–51 rests on an analysis of 304 Chester County inventories. But any single source suggesting a pattern of growth must be checked with care against other indexes. Price movements for grains suggest a slower pace. And



Gary Walton and Duane Ball's study of agricultural productivity in Chester County (*Journal of Economic History* [1976]) also indicates a far weaker expansion than Schweitzer alleges.

Schweitzer's description of the links between legislative initiatives and Pennsylvania growth can also be challenged. Her analysis of the workings of the loan office is excellent, perhaps the best part of the book. But her assertions about the impact of these loans on growth are not proven. Some individuals benefited from loans offered at low interest rates. But many others had to pay for these subsidies with higher taxes. Similarly, was the stability of Pennsylvania paper money a cause of growth (as Schweitzer asserts), or was the stable currency the result of a strong economy? More broadly, Schweitzer ignores the external causes of growth: the expansion of markets for grains and the willingness of Britain to extend credit and ship low-cost manufactures. Hence, the impact of legislative measures looms larger than it might in a more balanced analysis.

Despite some questionable assertions, this study is valuable. It rests on a strong evidentiary base, asks important questions, and strengthens our understanding of the colonial economy.

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WILLIAM E. FOLEY. *The Genesis of Missouri: From Wilderness Outpost to Statehood*. Columbia: University of Missouri Press. 1989. Pp. x, 367. Cloth \$32.00, paper \$17.95.

A generation ago an exceptional American historian, Louis Atherton, presided over the graduate education of a promising group of students at the University of Missouri. Since then the group has compiled a record of achievement that attests to Atherton's skill and knowledge. Among these scholars, none is more accomplished than William E. Foley. Foley's latest contribution to western studies exhibits his mastery of the early history of his native state and region.

Although based on his original *History of Missouri*, first published in 1971, *Genesis of Missouri* is no simple rehearsal of the earlier account. Considerably expanded, and incorporating Foley's extensive research and publication in the intervening period, it is a lively, current, and informative account of the pioneer years of Missouri's development. As the subtitle suggests, the period treated is from the earliest European exploration to the tumultuous admission of Missouri to American statehood, an event that prefigured the national crisis over slavery.

The book begins with the story of the area's American Indian tribes, such as the Missouri and the Osage, and moves on to describe the impact of the European arrival on Indian society. There follows a tight narrative of the French and Spanish era, the late

seventeenth century to the Louisiana Purchase of 1803, which traces the enduring impact of the French and Spanish on the region. Using recently published research on explorers such as the Sieur de Bourgmont, religious leaders such as Father Dubourg, mercantile figures such as Auguste and Pierre Chouteau and Manuel Lisa, miners and adventurers such as Moses Austin, and political personalities such as the Baron de Carondelet, Foley displays the complexities of cultural interaction in a wilderness setting. Careful to avoid the condescension of the past, he gives credit and criticism where it is due to Indian, Frenchman, Spaniard, African, and, finally, American settler in the evolution of a frontier society.

In his preface Foley notes that Missouri's early history may be regarded as the working out of a cultural identity. This seems true for the pre-American period, when Europeans altered the cultural landscape with their trade goods, religion, books, ideas of government and justice, and social organization, but it is equally true for the critical decade and a half between the American annexation and the admission of the Missouri territory to statehood. The struggles over governmental organization, free trade versus government control of Indian commerce, territorial defense, and the extended conflict over land claims dating to the Spanish period are all placed in the context of the meeting and merging of cultural elements. Eventually, of course, the Anglo-American element prevailed in imposing its political, social, and economic system on the emerging state, by sheer numbers if not otherwise. But Foley, while faithfully reporting the historical results, does not neglect the dialectic by which those results were obtained. The product is an informative and scholarly history that emphasizes and celebrates the diverse human elements whose lives constituted the genesis of Missouri.

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BILLY G. SMITH. *The "Lower Sort": Philadelphia's Laboring People, 1750-1800*. Ithaca: Cornell University Press. 1990. Pp. xiii, 249. \$34.50.

Billy G. Smith has carried a lantern through the workshops, cellars, taverns, and back alleys of the poor neighborhoods of late eighteenth-century Philadelphia, and along the way he has witnessed the shadowed lives of many thousands of the city's workers. He has taken special account of the merchant seamen, laborers, shoemakers, and tailors who made up "at least one-third and more often near one-half" of the city's free workingmen (p. 6). Smith's adventures into the dark corners of a great and bustling commercial city have resulted in one of the most detailed, sophisticated, and powerfully illuminating studies ever written about working people in early America.

The book begins with a literal tour de force. In the



first chapter, the author imaginatively reconstructs the routes taken by a man and a woman in 1790 "as they walk the streets searching for employment and carrying out some of their routine daily tasks" (p. 6). Their journeys featured not only the sights of mansions and poorhouses, the sounds of peddlars and mobs, and the smells of tanyards and sewers but also the central social and economic characteristics of the city.

It is impossible in a short review to do justice to all of the book's findings. But there are insights aplenty on urban demography and economy and on the employment paths, migration patterns, and family lives of working men and women, black and white. Smith reveals the violent, unpredictable environment of disease in which these people lived, emphasizing the class-specific experiences of health (and hence the basic demographic processes) within the city. He also constructs household budgets and compares them to wage rates in order to measure living standards; he finds that a great many working-class families "frequently found it difficult to earn enough money to meet their basic expenses" (p. 108). Moreover, the "vast majority of Philadelphia's laboring people were without property at any given point during the late eighteenth century" (p. 133). Smith's bleak portrait emphasizes the poverty, unemployment, limited upward mobility, inequality, and extreme insecurity that prevailed among "the lower sort." His principal conclusion is that "the income, living standards, and economic and occupational mobility of the lower classes [of Philadelphia] did not improve during the century's last fifty years" (p. 91). The moral exhortations of Ben Franklin and many historians after him notwithstanding, most Poor Richards remained poor.

Smith has used innovative methods to produce arguments that are precise, subtle, and sensitive to change over time. And to his credit, he has successfully resisted the temptation to make fetishes of those methods. Indeed, one of the many values of his book is the implicit critique it poses to the vast majority of quantitative historical studies, which have tended to be dull, dry, excessively abstract, and lacking in material context. Smith's work is humanized and enlivened by a strong sense of place (the streets and neighborhoods of Philadelphia), a rich knowledge of plebeian work practices, and an apt appreciation of the sheer difficulty of life for "the lower sort." Perhaps most important, Smith refuses the standard social scientific practice of separating social fact and social value, preferring instead to embed his quantitative analysis (of inequality) in broader political and ethical categories of understanding (of injustice). Questions of power remain central to the book despite its limited discussion of popular consciousness, agency, and politics.

Smith's conception of history "from the bottom up" brings to mind the 1960s and early 1970s, but his conclusions eerily echo the 1980s, especially when he

discusses the misery and oppression that lay at the heart of Philadelphia's "general prosperity" (p. 198). Historians will long use both the information and the interpretations offered in this book, which is to say that Smith has produced a work of genuine excellence and enduring scholarly value.

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JOHN PHILLIP REID. *The Concept of Representation in the Age of the American Revolution*. Chicago: University of Chicago Press. 1989. Pp. viii, 251. \$32.00.

John Phillip Reid's many volumes on the legal world of the American revolutionaries are not original, but that is their strength. He has recovered a body of ideas and ways of thinking about those ideas that successive waves of "progressive," "consensus," and "New Left" historians of the revolution have ignored or dismissed. Not since Charles McIlwain and Andrew McLaughlin labored, nearly a century ago and never with such loving attention to detail, has an American historian explored this universe of legal and constitutional usages.

Reid repeatedly warns his readers that the "old constitution" is not like our own. Twentieth-century Anglo-American law and jurisprudence concerns sovereignty and command. In the world of our constitutional fathers, he argues, colonial and British politicians and jurists believed that the constitution they shared was based on custom, not power; fundamental law, not absolute sovereignty; a limited government, not an all-pervasive state. In this old constitution, representation was not equal or direct, but neither was it virtual. The independent member of Parliament was the ideal not because the people conceded the privilege of rule to the elite or because Parliament claimed to be absolute but because representation nestled in a context of "shared interests, shared burdens, and equal assessments" (p. 132). The Commons was representative when it acted in a responsible way to limit abuses of power, not because it was fairly apportioned or because its members solicited the instructions of their constituents. When defenders of British government claimed that it was founded on the "consent" of the governed, they had in mind a consent that was corporate, not individual. Consent rested on the assumption of a mutuality of interests among the propertied electorate and the independent member of Parliament.

Although accountability was a watchword of this system, members of Parliament were not accountable to American colonists in any way. In the revolutionary crisis, long-held assumptions about the customs of representation collapsed, driving Americans to articulate a new conception: actual representation, instruction by constituents, and apportionment of legislatures based on population. In fact, American electoral and legislative practice already embodied

these notions; the crisis had merely forced Americans to see clearly, for the first time, how far they had already diverged from the English model. The dispute between British authorities and American revolutionaries concerned legal accountability, and the colonists "rebelled because they believed that parliament was violating basic constitutional precepts . . . English rights" defined in terms familiar to all the parties (p. 4). Rational discourse, not paranoia and naked self-interest, marked both sides' positions.

Reid's approach to the history of ideas is morphological. He is interested in structures, not dynamics. He thus draws from a broad temporal range of English and American writings in law, politics, and jurisprudence to construct his thesis, and he moves freely back and forth from our own time to the seventeenth and eighteenth centuries. He uses squibs from a variety of authors, sometimes pulling together many bits and pieces to make a single point. He skips across the Atlantic Ocean like Neptune, finding evidence for his arguments on both sides. Overwhelmingly, his sources are English, but he does not ignore the provinces. His chapters on consultation and instructions make superb use of these adversarial methods, although I have some reservations about the brevity of his other chapters. His style is spare, sometimes trusting the reader to know what he has said elsewhere.

Reid's technique of argument is one familiar to modern lawyers even if the substance of his arguments may not be, for in a very real way he has completed the briefs for the revolution that the revolutionary lawyers, in their haste, could not finish. Like them, he has an abiding faith in the efficacy of law and legal argument and the strength and value of common law.

PETER CHARLES HOFFER  
University of Georgia

LAUREL THATCHER ULRICH. *A Midwife's Tale: The Life of Martha Ballard, Based on Her Diary, 1785-1812*. New York: Alfred A. Knopf. 1990. Pp. x, 444. \$24.95.

With the publication of this book, Laurel Thatcher Ulrich has once again demonstrated her ability to re-create an engaging story that makes history come alive for readers. Using the long-neglected diary of Martha Ballard as the basis of her study, Ulrich pieces together a fascinating account of women's lives and work in preindustrial America. Whereas most historians have dismissed Ballard's diary as "trivial and unimportant," Ulrich shows that "the problem is not that the diary is trivial but that it introduces more stories than can be easily resolved and absorbed" (p. 25).

Beginning in 1785 at the age of fifty until shortly before her death in 1812 at the age of seventy-seven, Ballard kept a daily diary in which she recorded the events of her household and her life as a midwife on

the Maine frontier. In about 1904, an abridged version of the diary was prepared by Charles Elven-ton Nash as part of a proposed two-volume history of Augusta, Maine. Not until almost sixty years later, in 1961, was Nash's work, including about a third of Ballard's diary, published as *The History of Augusta*.

It is the original diary, not Nash's condensed version, that concerns Ulrich and that serves as the focal point of this book. In fact, Ulrich begins each of the ten chapters with long, unabridged passages from the original diary. These passages are followed by interpretive essays in which Ulrich skillfully incorporates information from a wide variety of materials ranging from wills, tax lists, deeds, court records, and town-meeting minutes to medical treatises, novels, religious tracts, and the papers of Maine physicians. By juxtaposing the actual diary entries with the interpretive essays, Ulrich helps twentieth-century readers to discover the larger meaning of the diary while also providing examples of how "important" material is often "submerged in the dense dailiness of the complete excerpt" (p. 34).

The breadth and depth of significant commentary found in Ballard's diary is astounding. We learn about early American courtship, marriage practices, and sexual mores as well as quilting bees, weaving, gardening, and tending to livestock. Ballard's diary reveals that her household consisted of two separate but cooperative family economies in which her husband traded lumber with landowners and merchants while she bartered cabbages and textiles with the wives and settled midwifery accounts with the men. Her diary contains a number of perceptive, albeit restrained, comments about family relations, rape, incest, premarital sex, illegitimacy, and even murder and suicide. Indeed, when one surveys the long list of topics that Ballard addressed, it is difficult to imagine how her diary ever came to be characterized as "trivial."

Ulrich tells us that Ballard was a midwife, nurse, physician, mortician, pharmacist, and attentive wife as well as a keeper of vital records of her town. It is Ballard's detailed account of her life as an early American midwife, however, that is perhaps most significant. With the exception of the last two decades, very few midwives during any period of American history left written testimonies. It is all the more remarkable, therefore, that a midwife's diary, written some two hundred years ago, has survived.

During the twenty-seven years that Ballard kept her diary, she carefully recorded each of the 814 births she attended. She witnessed only five maternal deaths. Given the times in which she lived, this was an admirable record. Not until the last fifty years has the United States' maternal death rate been reduced to one lower than that of Ballard's rate.

Ulrich has provided an astute analysis of Ballard's diary. More important, she has proved that historians

have much to learn from the "trivial" writings of "ordinary" women.

JUDY BARRETT LITOFF  
Bryant College

CHANDOS MICHAEL BROWN, *Benjamin Silliman: A Life in the Young Republic*. Princeton: Princeton University Press. 1989. Pp. xvi, 377. \$29.95.

As the first professor of chemistry and mineralogy at Yale College from 1804 to 1853, Benjamin Silliman (1779–1864) became a leading figure in the growing scientific community of nineteenth-century America. One of the founders of the Yale Medical School, editor of the *American Journal of Science* from its founding in 1818 until 1838, widely traveled lecturer, and early path maker in the field of scientific consulting, Silliman left an imprint on his times appreciatively noted by historians. Biographers in both the nineteenth and twentieth centuries recorded his life, and more specialized works have explored his contributions to science. In this volume, Chandos Michael Brown seeks not to add to the literature on Silliman as a scientist but to offer a cultural biography of Silliman during his youth and the years to 1820. The period after 1820, when Silliman had his greatest influence in the national scientific community, is reserved for a later volume.

The subtitle accurately reveals the author's focus and intent. The book presents a close and probing study of a young New Englander and his extended, often troubled, family in a changing world. In an imaginatively conceived and gracefully written book, Brown provides an extraordinarily revealing look at Connecticut society in the new republic and offers keen insights into the culture of the young nation.

Born during the revolution, while his father, a commander of a Connecticut brigade, was a prisoner of war in British hands, Silliman was sent to Yale at the age of thirteen. He graduated in 1796 in the first class to come under the influence of Timothy Dwight. His father then deceased and family fortunes declining, Silliman faced an uncertain future of dwindling economic opportunities in the region of his birth. Searching for his place in the world, he managed his mother's farm for a year, tried teaching school, and studied law before being appointed a part-time tutor at Yale by President Dwight.

In 1801 Dwight surprised his young favorite by asking him if he would like to become professor of chemistry. It was a field about which Silliman knew nothing, but he seized the opportunity. Aided by Dwight and the Yale Corporation, he was soon widening his world by studying in Philadelphia, observing the teaching of chemistry at Princeton, and traveling in Europe to purchase books and equipment. In 1804 he gave his first lecture at Yale as professor of chemistry and mineralogy to a class that, among others, included John C. Calhoun.

Brown skillfully and perceptively employs the rich resources available to illuminate his subject's life and thoughts. He has been diligent in seeking out family papers and letters retained by the circle of Silliman's correspondents, and he could not have written so revealing a book had not Silliman himself been so introspective and given to recording his thoughts, doubts, and deepest feelings. His journals, poems, essays, and other early writings are effectively employed. Silliman also kept journals of his travels and experiences in Europe. These were published and enjoyed considerable success. He later wrote reminiscences of his life. All are perceptively used by the author.

Details of family history are effectively presented and add richness to the volume, although they make demands on the reader that occasional reminders of relationships would have alleviated. The reader, however, is aided by the author's illumination of the culture of the society in which Silliman matured. The insights into religious life, economic life, and technological development are plentiful. The political background against which the events of Silliman's life transpire is only broadly indicated. Silliman's involvement and interest in politics seem to have been weak; even the War of 1812 appears to have only marginally disrupted his life.

Brown has succeeded in presenting an insightful portrait of a life in the young republic, deepening our understanding of the early decades of the new nation.

NOBLE E. CUNNINGHAM, JR.  
University of Missouri,  
Columbia

JOHN F. KASSON, *Rudeness and Civility: Manners in Nineteenth-Century Urban America*. New York: Hill and Wang of Farrar Straus and Giroux. 1990. Pp. xii, 305. \$22.95.

The disgust with which sophisticated European travelers reported such American habits as tobacco spitting has been noted by historians as evidence of a perception of the New World as rude and the Old World as civilized. In his discussion of rudeness and civility in nineteenth-century America, John F. Kasson moves the discussion of American manners far beyond this well-worn schema. As Kasson demonstrates, manners became an integral part of the definition and projection of social boundaries and identity, playing an essential role in the transformation of behavior and consciousness in an urbanizing, industrial age.

Kasson begins by recognizing nineteenth-century gentility of manners as a break with a distinctly cruder colonial code of behavior. This new gentility clearly reflected the class and gender assumptions of the era. A chapter on public entertainments, for example, argues that as these entertainments bifurcated into highbrow and lowbrow forms, their class

character was defined as much by the prescribed deportment of the audience as its composition. But etiquette did not merely reflect contemporary mores. It also tried to resolve the tensions inherent in a mobile, democratic, and acquisitive market society. Kasson's decoding of the Victorian formal dinner is a stunning example of this function. In the behavior of hosts, guests, and servants and in the very manner in which food was served, the dinner party functioned as a ritual of self-restraint in an age of materialism and greed, dangerous social appetites.

Kasson also argues that as a rank-structured, differential society gave way to one in which the market defined social relations, the "laws" of etiquette provided a new source of social authority. For individuals, acquisition of etiquette-book manners bought a secure place in the genteel middle class. The fact that manners were a purchasable commodity, however, pointed up one of the deepest tensions of the new urban and industrial order, namely, the troubling discontinuities between manners and morals, between superficial, multiple identities and the transcendent, immutable core of character. A pivotal chapter addresses this issue of social identity. Using sources from Allan Pinkerton to Edgar Allan Poe (one of the pleasures of this book is the intelligent eclecticism of its sources) and addressing such diverse issues as the "mysteries and miseries" genre of urban description, the confidence man as a character type, and the "science" of physiognomy, Kasson examines the pervasive anxiety experienced by the Victorians as they confronted the potential of social deception in an anonymous, commercial society. Etiquette manuals responded to this chilled fascination by educating their readers in the semiotics of everyday behavior, providing the means to distinguish social counterfeits from the genuine article.

Here Kasson is at his most daring and productive, moving well beyond a bemused interest in Victorian social conventions to a penetrating analysis of what Kasson labels the "crisis of social representation" (p. 117). What could have lent his wide-ranging explorations even greater internal coherence is a discussion of the boundaries of the field of etiquette as defined by etiquette manuals themselves. That these manuals addressed such issues as the reading of character indicates that the issue of self-representation is no historiographical construct, that Kasson's gift has been to recognize the inherent structure of Victorian etiquette. Additionally, other issues, such as the nature of proper business and personal correspondence, might have been suggested as the subjects of fruitful analysis.

Apart from the original conceptualization and insightful analysis, even apart from the engaging writing style, one cannot help but delight in the pungency of detail Kasson provides. The symphony conductor as audience disciplinarian, the popularity of urban bird's-eye view lithographs, the hidden meaning of a folded calling card—these are the kinds of subjects

that make Kasson's book as entertaining as it is illuminating.

TAMARA PLAKINS THORNTON  
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Fredonia

ROSS THOMSON. *The Path to Mechanized Shoe Production in the United States*. Chapel Hill: University of North Carolina Press. 1989. Pp. xii, 296. \$39.95.

In this thorough study of nineteenth-century industry, Ross Thomson compels a reexamination of our easy generalizations about the generation and diffusion of technological change. He addresses two issues: first, how do craft-oriented societies mechanize, and, second, once launched, how does mechanization become self-generating? Although the author draws on a plethora of studies of the shoe industry, the heart of his research comes from a close analysis of thousands of shoe patents. These lead him to offer a new lexicon for comprehending what economic historians consider to be the preeminent characteristic of the industrial revolution, namely, pervasive mechanization.

In the first section of this volume, Thomson explores a question left unanswered by Karl Marx and Paul Mantoux. How does the transition from handicraft to machine manufacture take place? Rather than posit a sharp break, Thomson lobbies for a transformation within the craft system that made mechanization possible. Two institutional changes predominate. The emergence of a national market for shoes in the first half of the nineteenth century facilitated the clustering of firms in eastern Massachusetts and their shift to more thorough task-oriented divisions of labor. Second, this concentration of shoe firms in a region of machine makers encouraged technological cross-fertilization, bringing forth a new product fit for the national market, the standardized, pegged shoe. Although shoe production remained essentially a handicraft business, the institutional groundwork had been laid for a second, industrial stage.

This stage was characterized by the emergence of specialized shoe machinery companies, which revolutionized the industry. These new capital goods firms had no antebellum predecessors, and, although a sizable market beckoned, they had to surmount technical, production, financial, and marketing barriers. The sewing machine, developed for another industry, offered the entering wedge, or "break-in" opportunity. Originally used to sew uppers, it was modified by Lyman Blake and later Charles Goodyear into a wet thread machine that could stitch bottoms. Both types of machines involved significant technological breakthroughs, because they did not replicate craft processes. These innovations fostered the growth of specialized shoe machinery firms, which built rudimentary machines, perfected them through use, and diffused them through sale or lease. Once the ma-



chines reached the market, a virtuous circle ensued, as sales spread learning and encouraged further innovation. This "learning by selling" not only underwrote the self-generating aspect of mechanization but also made this mechanization "path dependent" (pp. 116, 243). An analysis of the shoe industry's patent history, particularly the prepatent occupations of inventors and the probability of repeat patents, permits Thomson to argue that innovation came primarily from within the industry. In short, the author insists on a perspective that is specific to product, market, and process in accounting for mechanization.

Historians will find this analysis congenial. It conforms to the emerging consensus that little meaningful mechanization came before the Civil War, although the pace of manufacturing quickened. It focuses on institutional or social transformations that mediate between the stages of industrialization. And it insists on a much more complex account of how machines were invented, diffused, and refined. Thomson explicitly criticizes the economists' perspective that focuses on aggregates such as the growth of demand, the supply of skills, and the price of capital goods. He contends that technological change across the economy was the sum of relatively distinct cumulative processes in different industries rather than an induced response to generalized shifts in demand or supply. But if he has identified variations in the pace and pattern of mechanization, he has not accounted for the pervasive biases that characterized American industry in this second stage. The leather goods industry, like most manufactures in the nineteenth century, underwent persistent changes in technique, changes that enabled the industry to substitute cheapening capital goods for dear labor.

DIANE LINDSTROM  
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JEAN FAGAN YELLIN. *Women and Sisters: The Antislavery Feminists in American Culture*. New Haven: Yale University Press. 1989. Pp. xxi, 226. \$25.00.

Jean Fagan Yellin is a professor of English, but she brings to her highly original subject—a history of visual symbols used by antislavery feminists—all of the instincts of a dyed-in-the-wool historian. In the best American studies tradition, she slices into nineteenth-century feminism from a unique angle, tracing the meaning of a potent icon, the shackled, kneeling slave woman, from its early uses in the 1830s, through several transformations and reconfigurations by a variety of feminists black and white, to its later appropriation by antifeminist authors and artists. Yellin grounds her discussion of encoded icons and the discourse of racial and gendered power relations in detailed accounts of the lives and ideas of

the women and men who interpreted the symbol so variously.

The icon itself is a familiar one: the seminude black woman, enchained and kneeling, sometimes accompanied by a standing white woman. William Lloyd Garrison used it as a masthead for the "Ladies Department" section of *The Liberator*, accompanied by the words "Am I not a Woman and a Sister?" Versions of the logo appeared on antislavery artifacts, from personal stationary to needlework patterns. In 1845, a Boston foundry produced it as a typographers' design; the logo had become "literally a stereotype," Yellin notes (p. 23). Yellin has culled hundreds of instances of its use, moving from cartoons, frontispieces, and newspaper graphics, to its three-dimensional representation in ceramic and stone sculpture, and finally—and most boldly—to its use as a figure of speech in written texts.

Yellin explores the different ways that early antislavery feminists such as the Grimke sisters and Lydia Maria Child interpreted the icon. Angelina Grimke clearly identified with the slave woman; all women are slaves, demanding to be freed. Child saw herself as the standing white woman, a public woman representing both a challenge to patriarchy and a liberator of the enslaved. But sometimes antislavery writers interpreted the standing white woman as the embodiment of true womanhood, lifting the tragic mulatto victim into domesticity and sisterhood. Not surprisingly, Sojourner Truth and Harriet Jacobs read the configuration quite differently; they did not confuse the literal chains of black women with the figurative chains of whites, nor did they focus on the power of white liberators.

By the 1840s and 1850s, the icon still had instant emotional impact, but it began to be coopted and drained of its original feminist meanings. Yellin examines Hiram Powers's sculpture, *The Greek Slave*, and Nathaniel Hawthorne's treatment of the woman in shackles in his invention of Hester Prynne of *The Scarlet Letter*. In both cases Yellin delves deeply into Powers's and Hawthorne's backgrounds (Cincinnati and Salem) to demonstrate the context in which each man learned about antislavery and feminism. She argues that they were familiar with the feminist content of the logo—women protesting enslavement—but repackaged it to present women as passive victims. A final chapter tackles Henry James's *The Bostonians*, with its cruel parody of antislavery feminists. Yellin loses her theme of the enchained woman here and instead seems to be using James to demonstrate the silencing of antislavery women through ridicule.

This volume is richly illustrated, and the text carefully explicates each picture. Yellin shows the power of visual symbols to convey as well as to shape and structure the way people understood patriarchy and slavery. Her work is truly interdisciplinary, and it presents a remarkably inventive way to get at how nineteenth-century people, both radicals and tradi-

tionalists, thought about the intersection of race and gender.

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JANET L. CORYELL. *Neither Heroine nor Fool: Anna Ella Carroll of Maryland*. Kent, Ohio: Kent State University Press. 1990. Pp. xv, 177. \$22.00.

Anna Ella Carroll (1815–94), descendant of a distinguished Maryland family and daughter of a politician, fed a lifelong addiction to politics in the only way open to women of her day, namely, by becoming an essayist, lobbyist, critic, hostess, propagandist, and political gadfly. Although not an appealing or endearing individual—she was given to shameless self-promotion, pretensions, and exaggeration—her story does reveal a highly intelligent woman who, through the years, played a legitimate, albeit relatively minor, historical role by promoting her father's career, writing for the Know-Nothing party, and supporting presidents Millard Fillmore and Abraham Lincoln. During the 1850s her skills as essayist and lobbyist did admit her to the circles of power in Washington, D.C., and her fascination with the ideology of Manifest Destiny and the ideals of American government fueled her patriotism and her writings.

Her most important contributions were as an exponent of the nativist and anti-Catholic Know-Nothing philosophy and later as the author of a widely read set of pamphlets that supported some of Lincoln's most controversial war policies. But, despite her prolific writing, history remembers her not as an essayist or propagandist but as the woman who claimed that she, and she alone, was the true mastermind behind the Union plan that led to the successful invasion of the Upper South via the Tennessee River, a strategy that ultimately split the Confederacy in half. After the war she spent the rest of her life in a most unbecoming and self-serving drive to get financial reward and recognition from the federal government for this claim. It is concluded that, although she helped disseminate the plan, it was such an obvious strategy that numerous individuals rightly could claim to have come up with the idea, including an obscure colonel in the U.S. Army Corps of Engineers, a riverboat pilot, and General U. S. Grant himself.

The final chapter of this book traces Carroll's historical legacy once her claim and cause were taken up by nineteenth-century suffragists and feminists. This part of the story is the most interesting to modern readers, for, as the author states, the waxing and waning of Carroll's place in history seemed to mirror "women's place in American society and the regard in which women's history was held throughout the twentieth century" (p. 110). Janet L. Coryell shows, too, that Carroll's life is a classic case of the nineteenth-century woman of letters who faced over-

whelming societal odds once she dared to venture beyond the home and beyond the confines of the cult of true southern womanhood.

This well-written, well-researched biography, which is enhanced with excellent notes and bibliography, is a welcome addition to the literature of women's history and especially to the branch that specializes in nineteenth-century America.

LINDA VANCE  
Austin, Texas

SALLY G. MCMILLEN. *Motherhood in the Old South: Pregnancy, Childbirth, and Infant Rearing*. Baton Rouge: Louisiana State University Press. 1990. Pp. xi, 237. \$24.95.

The assumption underlying Sally G. McMillen's study is that pregnancy, childbirth, and infant nurture had distinctive social characteristics in the Old South. Using published collections but depending primarily on manuscripts in repositories in North and South Carolina, Tennessee, and Arkansas, McMillen examines the maternal experience of some seventy-five to one hundred privileged white women who bore children between 1800 and 1860 and who were married to planters, merchants, educators, ministers, and physicians. Her ancillary theme, the medical treatment that these women received, derives from southern medical journals and medical students' dissertations and from handbooks published largely in the North. Elitist as its sources may be, McMillen's research should have produced a more broadly convincing analysis than that offered here.

What went wrong? First of all, sweeping generalizations often lack the statistical evidence or relevant citations needed to make them credible. If we do not know that upper-class southern families were, in fact, larger than and thus inherently different from comparable northern families, why should we believe the explanation that this was so because "few social or economic constraints limited [Southern] fertility" (p. 3)? Similarly, when table 7, adduced to demonstrate the high frequency of child mortality in the South, records that the highest death rates for children under five occurred in recently settled states in the Midwest and Far West, and in states with large urban concentrations, the reader must wonder whether the cogent factors are not differences between settled and frontier areas or between city and country rather than between the Old South and either the North or the rest of the United States.

In addition, McMillen's concluding assessments, often coming at the end of individual paragraphs, are frequently so indefinite, so replete with "maybe" and "perhaps" that they further erode reader confidence. Some treat only minutiae—the paucity, for instance, of references to slave attendants in the birthing room: "That may be because none was present, because the slave's status made it inappropriate to mention her in

a letter, or because the presence of black women was exceedingly common" (p. 67). More broadly, the central thesis that "it is possible that mothers in the antebellum South developed a deeper commitment than women in other regions to family and maternal responsibilities" (p. 170) is pressed in an intellectual vacuum that ignores all but those responsibilities. What, for example, of the managerial obligations of planters' wives, the religious and charitable activities of town-dwelling lawyers' and merchants' wives, or the extensive reading reported in the letters of some of these highly literate women? Dare one conclude on this basis that privileged southern women "had no choice but to devote their lives to mothering" (p. 170)?

The contribution of this study, therefore, is largely its descriptions of how individual women, their kin, and their doctors responded to childbirth and early motherhood. Yet even here problems arise if the evaluation and use of the R. F. W. Allston papers is representative, for nephews are labeled adopted children, names of correspondents are incorrectly given, and the great Georgetown planter and governor of South Carolina, who was Adele Petigru Allston's husband, is identified only as a Charleston resident.

In sum, one wishes that either at the dissertation or the editorial stage someone had subjected this first book to the rigorous standards that in recent years have made women's history a disciplined and highly innovative branch of social history.

JANE H. PEASE  
College of Charleston

JAMES OAKES. *Slavery and Freedom: An Interpretation of the Old South*. New York: Alfred A. Knopf. 1990. Pp. xxi, 246. \$22.95.

"Southern slave society grew up in a liberal capitalist world," argues James Oakes in this provocative study, "and never for one moment escaped its influence" (p. 39). In a series of "essays in interpretation, frankly exploratory and by intent suggestive rather than definitive" (p. xx), the author develops this thesis and its consequences. In so doing, he engages the most fundamental questions of southern history—the structure of slave society, the character of the master and slave relationship, the economics and politics of slavery—and the historians, most notably Eugene D. Genovese, Kenneth M. Stampp, and Edmund S. Morgan, who have grappled with them. Although the nature of the enterprise raises more questions than it can possibly answer, there can be little doubt that Oakes's hope to "stimulate discussion" (p. xx) will, as this ambitious and brilliant argument is examined, be realized.

Oakes begins by describing the seventeenth-century world that capitalism made for the slaveholders. In his second chapter, a gem of an essay that might well stand alone, he explains how, by the time of the

revolution, southern planters were fully a part of the world of Atlantic commerce. In this economy, consumption set the rhythms, and the boundaries, of production. Slavery "was itself the servant of the driving force of capitalism" (p. 52). A growing demand for consumer goods set this economy apart from that of Greece or Rome: "American masters were the first in history whose power depended on commercial relations with a capitalist world that was ultimately more powerful than all the slave societies put together" (p. 53). Plantation agriculture thus became, in fact, "pawn, not king" (p. 56).

At the same time, within this capitalist world, there developed the liberal state, in which the rights of the individual held primacy. The American strain of liberalism as understood by slaveholders was not incompatible with slavery. Indeed, by the nineteenth century, when liberalism was well developed, the slave codes had become most specific, denying point by point any claim a slave may have to security, liberty, or property. Slavery reinforced liberalism, Oakes contends, "by specifying what it meant to be unfree" (p. 69). Placed by law not at the bottom of the social ladder but entirely off it, the slave served as a daily reminder of the inalienable right of those who were not slaves to life, liberty, and property. Unlike aristocratic societies, where an upper class might claim special legal or political privileges, the slave South professed subscription to a body of universal rights from which "a class of slaves was carved out and excluded" (p. 77). Racism became a reinforcing principle of this exclusion for a society that, although it chose planters in disproportionate numbers for public office, was dominated not by planters but by an applied liberalism that made all white men equal.

Such an interpretation of the prewar South forces a reconsideration of the postwar years. Ironically, Oakes points out, the defeat of the Confederacy produced the beginnings of the planter hegemony that some historians have discerned in the antebellum era. Where white liberalism had forced planters to accommodate themselves to the economic and political demands of nonslaveholding whites in the years before 1861, after 1865, and particularly after reapportionment of the legislatures gave greater representation to heavily black districts, opposition to black voting led first to black disfranchisement and then to the "political rise of the planter class" (p. 201).

Oakes concludes by lamenting the persistence of the dividing line separating scholars of social history and scholars of politics. The present work seeks, with considerable success, to erase that line, even as it takes positions on those issues that have defined the "civil wars raging among [southern history's] most accomplished chroniclers" (p. 209). Clearly written, compellingly argued, rigorous and challenging, this book is apt to take its place alongside the most accomplished of those chronicles.

JOHN MCCARDELL  
Middlebury College

DON E. FEHRENBACHER. *Constitutions and Constitutionalism in the Slaveholding South*. (Mercer University Lamar Memorial Lectures, number 31.) Athens: University of Georgia Press. 1989. Pp. xiv, 115. \$16.00.

In this brief volume, Don E. Fehrenbacher focuses his immense knowledge of antebellum political and constitutional history on the South and constitutions. Yes, constitutions, for Fehrenbacher treats southern state constitutions, the Confederate constitution, as well as the southern relationship with the federal constitution. Fehrenbacher worried that including all three in his survey might have been too ambitious. He worried needlessly. He has written a cogent account that is filled with useful information and sharply drawn interpretations.

Discussing state constitutions, Fehrenbacher notes the differences between them and the federal constitution. The latter, after the addition of the Twelfth Amendment in 1804, remained untouched until the Civil War. In contrast, state constitutions underwent not only dramatic and continuous revision but even replacement. In fact, only North Carolina retained its original constitution, with substantial amendments, of course. Fierce political battles over issues such as representation and banking reached beyond statutes into the constitutions. Such changes generated much "superlegislation"—that is, laws having statutory quality but constitutional status—and longer documents (p. 25). Fehrenbacher notes that events in the South paralleled those in the North. Thus, in its treatment of state constitutions, the South was very much like the North.

When he turns to the U.S. Constitution, Fehrenbacher concentrates on what he sees as the key question in antebellum state-federal relations. He points out that North and South did not always take opposite sides. Stressing that states rights did not become chiefly a southern doctrine until the 1820s, Fehrenbacher connects southern constitutionalism with southern politics. In his view the Missouri crisis, when majoritarian politics failed to protect southern interests, prompted the southern commitment to states rights. Though often tension filled, this twin allegiance to a "states-rights constitutionalism" and "the Jeffersonian strategy of majoritarian politics" remained dominant until the triumph of the Republicans routed that political strategy (p. 49).

The victory of the Republicans led to secession and the creation of the Confederate constitution, which Fehrenbacher finds both conservative and innovative. He emphasizes that the Confederate constitution makers not only retained a federal government but made clear its supremacy. Although Fehrenbacher points to the essential constitutionalism of the Confederacy, he recognizes that the war wreaked havoc on what southern independence was supposed to protect—"slavery, state sovereignty, and constitutional rights" (p. 80). Considering the old perennial Confederate nationalism, now blooming profusely,

Fehrenbacher maintains that the text of the Confederate constitution provides *prima facie* evidence of a powerful urge for an independent South. He equates the South in 1861 to the United States in 1776 with a common hostility to an alleged foe the binding force. Of course, what the Confederate constitution and the incipient Confederate nation would have become is unknowable; 1783 gave victory to the Americans, whereas 1865 meant defeat for the Confederates.

Throughout this excellent book Fehrenbacher keeps in sight the South as a distinct section and as part of the nation. Perhaps his chief contribution here underscores the power and force of that tension.

WILLIAM J. COOPER  
Louisiana State University

THOMAS E. JEFFREY. *State Parties and National Politics: North Carolina, 1815–1861*. Athens: University of Georgia Press. 1989. Pp. xiv, 422. \$45.00.

Like King Gordius's intricate knot, the second American party system is a historical puzzle that appears to defy solution. Unable to reach anything more than a temporary consensus on the nature of the system at the national level, scholars have turned to the states in the hope that local analysis will help to untie the riddle. In this definitive study of North Carolina politics, Thomas E. Jeffrey suggests that party differences were sharp and that these differences reflected profound disagreement over the direction of national economic development. But ever-present state sectionalism, combined with the fact that both parties had strong wings in both east and west, forced the parties to play on local issues. This intense competition over regional concerns meant that neither party could achieve permanent dominance and kept the two parties competitive long after the system had broken down on the national level.

One of the many strengths of the work is the author's decision to reach beyond the heyday of Andrew Jackson; despite the subtitle, the book stretches from the Revolution to the Civil War. What that sweep tells us about Carolina Whigs is especially significant. Aside from their support for Nicholas Biddle's bank and the distribution of public land revenues, Whigs of the Old North State demonstrated few other nationalist tendencies and publicly espoused the doctrine of states' rights. By arguing that their support for the bank did not indicate a general policy of federal activism, the Whigs were able to portray themselves successfully as defenders of economic modernization and slavery. As a result, the state party remained active long after the national party died. Following a brief foray into the Know-Nothing ranks, the Whigs reformed under their old name and were on the verge of again taking control of the state when the guns of Sumter carried North Carolina out of the union.

Jeffrey, the author of a number of articles on North



Carolina, knows the terrain well. Yet a study like this one raises as many questions as it answers; indeed, it carries us away from, not toward, a Jacksonian consensus. The author gently criticizes Harry L. Watson's community study by suggesting that Cumberland County might not be representative of North Carolina as a whole. But there is much evidence that Jeffrey's findings are not applicable to other states, including neighboring Virginia. The author concedes that the vitality and nature of the Whigs distinguished North Carolina from other states, and, if the rationale for a state study is to illuminate the national scene, North Carolina might not be the state to examine. More serious is the author's explication of political culture. After a few brief nods at Democratic alarm over the emerging market economy, a factor increasingly used as a way of explaining antebellum party allegiance, Jeffrey falls back on Marvin Meyers's outdated and simplistic assertion that the Jacksonians appealed to the paranoid fears and resentments of Americans. There is no hint that many Carolinians had good reason to fear the onset of an impersonal market society and the loss of their economic independence. These are minor caveats. This important work will prove useful to historians of antebellum politics and indispensable to scholars of the Old North State.

DOUGLAS R. EGERTON  
*Le Moyne College*

CLYDE N. WILSON. *Carolina Cavalier: The Life and Mind of James Johnston Pettigrew*. Athens: University of Georgia Press. 1990. Pp. xiv, 303. \$35.00.

As his title implies, biographer Clyde N. Wilson considers his subject, James Johnston Pettigrew, the very embodiment of the Southern Cavalier: intelligent, chivalrous, proud, and brave. Too long, argues Wilson, has this image been mocked or tarnished by northern historians who see nothing behind it but bluff and swagger. In Pettigrew at least—and Wilson implies that Pettigrew, although unique in many ways, was still a representative southerner—myth and reality were one.

Johnston Pettigrew, as he was called, the son of a wealthy North Carolina planter, was intelligent. A brilliant scholar, he left a record almost unmatched at the University of North Carolina, then worked with astronomer Mathew Fontaine Maury at the National Observatory, studied for a few years in Europe, and finally returned to pursue, half-heartedly, a legal career in Charleston, South Carolina.

Pettigrew was chivalrous, literally carrying ladies over mud puddles. He was proud, involving himself in the preliminaries of a half-dozen or so "affairs of honor," despite his own belief that the code duello was barbaric. And he was bold and brave, fascinated with danger, always seeking ways to test his personal courage. He found his opportunity at age thirty-three

with the outbreak of civil war. As a brigadier general commanding North Carolinian troops, Pettigrew was wounded four times, captured once, and killed finally in a minor skirmish after having participated in Pickett's celebrated charge at the bloody battle of Gettysburg.

Wilson's admiring, and almost entirely uncritical, portrait of Pettigrew recalls the views of Charles Beard and of the southern nationalist school of history. For Pettigrew is not only a knight in shining armor, whose flaws (excessive honor and courage) are really virtues, but also a representative of an earlier Jeffersonian republican ideal, an ideal that rejects the individualistic, acquisitive, money-grubbing ethos of the industrialized North. And so, when his adopted state of South Carolina left the Union in December 1860, he naturally followed but not for the reasons accepted by most contemporary historians. Wilson contends that Pettigrew gave his life for states' rights, not slavery.

Wilson also would have us believe that Jefferson Davis, who argued that the South seceded to maintain great constitutional principles, was a more representative southerner than the oft-quoted Alexander Stephens, who insisted that slavery was the cornerstone of the Confederacy. This, too, will be hard to swallow for readers who recall that phrases such as "states' rights" and "government by the consent of the governed" were often used as smokescreens for the defense of the South's peculiar institution of black slavery. Wilson ought to distinguish between stated motives and underlying causes.

Whether one agrees or not with Wilson's thesis, his final chapter, an analytic look at Pettigrew's life, is worth reading and is, in fact, the best part of the book. As a biography, this work is disappointing, filled with long block quotations that are necessary only to pad what should have been an article, not a book, and with the biographer's intrusions into the narrative, interrupting what should have been the sense of a life being lived.

BETTY L. MITCHELL  
*Southeastern Massachusetts University*

DAVID F. ALLMENDINGER, JR. *Ruffin: Family and Reform in the Old South*. New York: Oxford University Press. 1990. Pp. xiv, 274. \$34.50.

David F. Allmendinger, Jr., presents this book as "an effort to combine intellectual and social history, focusing on [Edmund] Ruffin's thought as it developed through his experience" (p. xii). Such experiments are welcome, since biography remains one of the least well understood approaches to coherent understanding of the past. Allmendinger's claims for what can be learned from a single life are modest, but his commentary on the purposes and responses of a prominent agricultural reformer and secessionist is insightful and well documented.

The book begins with a narrative of the suicide in 1840 of Ruffin's friend Thomas Cocke; it concludes with analysis of Ruffin's own suicide in 1865. The intervening chapters, mostly topical, on Ruffin's lineage, agricultural experiments, editing and writing career, relations with his children, business arrangements, and views on race and slavery refer frequently to fears of dependency and an unrelenting quest for control.

One term that explains much of Ruffin's behavior, according to Allmendinger, is "isolation." High mortality rates along the James River meant that "no comforting, sustaining circle of relatives from older generations" surrounded Ruffin as he grew up (p. 16). In his own lifetime, many children survived, which created a "miniature Malthusian crisis" (p. 28) within the family. As his projects in agricultural science and reform staved off this crisis and brought success and influence, Ruffin, who lacked much consciousness of family tradition, stood forth as a model of self-reliance.

In stressing isolation, Allmendinger may underemphasize cultural and intellectual influences and exaggerate the novelty of Ruffin's ideas and projects. But several interesting passages suggest parallels between Ruffin's enterprises and those of other reformers in the North and in England. In his determination to alter folk customs and material conditions, Ruffin was much less ambivalent about change than many contemporaries. Useful discussions of the business side of intellectual endeavor show Ruffin's resourcefulness in the marketplace. But the South was frustrating territory for a modern intellectual, and only a network of family-run farms secured economic independence and made a literary career possible.

In the 1850s Ruffin enjoyed the large audiences that even turgid tracts on secession attracted. As a reformer, he thought a slave work force indispensable to any large-scale program of replenishing the fertility of southern soil. Therefore, he discarded reservations about slavery that as a young man he had shared with intellectuals in other regions and nations. Just as some northerners castigated slave owners as enemies of free labor, Ruffin came to disparage black slaves as incapable of the self-discipline and moral improvement that he esteemed in his own life history. "In falling upon racial inferiority as a defense for slavery," Allmendinger points out, "this Southerner who possessed a world of practical experience was resorting to the abstraction of all abstractions in antebellum thought" (p. 147). Perhaps the real point illustrated by Ruffin's life is the deceptiveness of self-reliance in a society cut off from tradition and devoted to commerce. The final pages on the isolated old man's despair advert to themes of contradiction and futility that somehow seem to have been present all along.

LEWIS PERRY  
Vanderbilt University

HUGH DAVIS. *Joshua Leavitt: Evangelical Abolitionist*. Baton Rouge: Louisiana State University Press. 1990. Pp. xiv, 328. \$35.00.

Joshua Leavitt's career as an abolitionist stands in the high middle ground of antebellum antislavery advocates and general reformers. Never commanding the power of a William Lloyd Garrison, Theodore Dwight Weld, or Wendell Phillips, Leavitt nonetheless wielded special influence as an editor of various evangelical and reform publications and as an organizational insider. For historians, his life as evangelical, early abolitionist, and political antislavery activist opens a window on many of his generation's styles of reform commitment. Hugh Davis's biography possesses the cardinal virtue of amplifying and humanizing our understanding of each aspect of Leavitt's career without making undue claims for the unique character of its subject. Indeed, Davis constructs his life within a balanced appreciation of the basic historical literatures of each era in his life. Leavitt seems distinctive only in his rare combination of evangelical zeal and deep commitment to the political process. For the most part, the author renders him as an individualized exemplar of reform tendencies and conflicts, as well as the progression of reform interests from the 1820s to the 1860s.

In fashioning this public life, Davis has played to the strengths of the historical record. Leavitt left to posterity the raw material for such a study in numberless editorials and letters written in the heat of battle. The author weaves the insights of Leavitt's critical eye and distinctive voice in the retelling of such familiar stories as the anti-abolitionist rioting of the 1830s, the Presbyterian schism of 1837, and the split in abolitionists' ranks of the late 1830s. More valuable still, Davis enriches our understanding of Leavitt's views on a variety of issues as the evangelical editor entered antislavery's political arena and began to grapple with interconnected issues of slavery, economic policy, and government organization.

Alas, what we miss is a living portrait of the man. This is hardly Davis's fault, since even the Leavitt papers at the Library of Congress and Ohio Historical Society reveal little about the inner man. The brooding photographic portrait that graces the book's title page hints at depths that Leavitt's unrevealing pen and accidents of historical preservation have made impossible to plumb. This may be just as well, since Leavitt's importance to historians lies more in the world that he reflected in his writings than any that his idiosyncrasies may have helped to create.

In short, Hugh Davis's book adds one more full life to our general picture of antebellum reform. It is a welcome addition, clearly written and soundly researched. Although Davis has claimed no startlingly

new ground, he has added detail and nuance to an ever more complex, if familiar, picture.

ROBERT H. ABZUG  
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PATRICIA GRIMSHAW. *Paths of Duty: American Missionary Wives in Nineteenth-Century Hawaii*. Honolulu: University of Hawaii Press. 1989. Pp. xxiii, 246. \$25.00.

The eighty American women who accompanied the American Protestant missionaries to Hawaii, from the 1820s to the 1840s, almost all as wives, are the subject of this study. Patricia Grimshaw wishes to tell the human story as well as to bring new insight to Hawaiian history. She has written a moving account of women's lot in the period. The work is not, however, a history of the women in Hawaii.

These women grew to maturity in the United States when women were achieving a new role in society. The mission women came to Hawaii with their own separate agenda: they would be active participants in God's work. They intended to involve themselves completely in the task of Christianizing the Hawaiians: to teach, to conduct religious meetings, and to engage in extensive charitable pursuits.

Although many of the women were educated in the new seminaries in the United States, they were ignorant of alien and indigenous cultures and disinterested in learning about them. They suffered, as did most Americans of their time, from an ethnocentrism that elevated U.S. culture to an exaggerated status of superiority.

Grimshaw has used as her primary sources the diaries, letters, and writings of the women themselves. Such a dependence has meant that the author has taken at face value their own estimation of their failures and disappointments. Their inner searchings, so essential to the Calvinist psyche, are taken as the full reality of the period.

Mission women believed that they had failed to achieve any of their pious objectives. They became overwhelmed by the immediate needs of home and children. Wives were forced to give up teaching and conducting religious meetings by the pressures from "the gender division of labor" (p. 99). Nothing about their lives in Hawaii was pleasant. They were horrified by Hawaiian culture and Hawaii's people, deplored the climate and environment, saw nothing inspiring in the scenery, and did not make friends of the other foreign women residents. Suffering is the central message of this study. Indeed, the description of the women's physical and nervous disorders (pp. 48, 70, 75, 151) would lead the reader to question their emotional stability and mental health.

By failing to use a broad historical basis for her study, Grimshaw has seriously diminished the role of these remarkable American women in the history of Hawaii. She credits the missionary wives where they

were able to be "extremely influential" (p. 195), which was not in the service of the spiritual salvation of the Hawaiians but in the promotion of the economic materialism of their children. These women determined and succeeded in isolating their children from close relationships with Hawaiians and from knowledge of Hawaiian culture. Their efforts did preserve an interconnected group of missionary descendants who through marriage and business enterprises kept their isolation from the totality of life in the Hawaiian Islands.

It seems unfortunate that this study should emphasize these women's martyrdom, their cultural imperialism, and their racial bigotry. A study of them using broader sources of historical materials would show the depth of their success in imparting social, religious, and educational values rather than primarily material ones.

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RICHARD PLUNZ. *A History of Housing in New York City: Dwelling Type and Social Change in the American Metropolis*. (The Columbia History of Urban Life Series.) New York: Columbia University Press. 1990. Pp. xxxvii, 422. \$45.00.

Richard Plunz, an architect and faculty member at Columbia University's School of Architecture, has written a detailed survey of the many architectural innovations, disasters, and evolving developments in New York's housing history. Plunz defines New York as "a city of housing, not houses" (p. 4), pointing to a local historical preference for buildings that house groups in large structures rather than for those that house families or individuals in small separate structures such as private houses.

Plunz begins his survey with a recounting of the building laws that have been enacted to control New York housing, starting with a law in 1676 allowing the city to take over decrepit houses and convey them to new owners willing to do necessary repairs. He then describes and analyzes hundreds of fascinating examples of both built and projected housing forms, concentrating on those of the last one hundred seventy-five years. Plunz ends with a pessimistic view of the treatment of housing questions in the 1980s and the "social catastrophes" that result when housing is not seriously engaged as a crucial public issue (p. 340).

Plunz is concerned especially with the physical forms that housing has taken but also attends to the people for whom specific sorts of housing were designed. He looks widely and captures the diversity of dwelling forms, including Lower East Side tenements for the nineteenth-century immigrant poor, Bronx trade union apartment cooperatives of the 1920s, high rise towers on Central Park for the upper

middle class. The legal and financial structures that support each sort of building are brought into the story. The author is in favor of federal and state support for housing and is much more concerned about the health and welfare of residents than about aesthetic or formal issues in architecture.

Plunz's attention is focused especially on issues of the sites of multiple dwellings, pointing out the percentage of a site covered by a building, the configuration of a building's "footprint," the relation of a building's wall to the street, directional orientation, and the way each specific configuration accommodates the need of tenants on the inside for fresh air and sunlight. Plunz is less concerned with how the interiors of individual dwelling units were laid out, what kinds of rooms and facilities were provided, and who and how many actually lived in the examples he studies.

Plunz's book does not break new ground but very usefully compiles materials heretofore only available in scattered sources. For example, the tenement problem of the early nineteenth century, the first designed tenements, architectural competitions and changes in tenement law are covered elsewhere, but Plunz's contribution is to set these more familiar cases against developments for the well-to-do, such as Llewellyn Park, New Jersey, a designed commuter suburb exactly contemporary (1853–55) with the philanthropic Workingmen's Home for black tenants in Manhattan, and to compare tenements with non-designed housing for the poor such as shacks or shanties. At every chronological stage in the story, Plunz includes housing for a diversity of income levels and positions city housing against clients' opportunities for suburban choices.

Plunz's treatment of issues in social history is less adept than his analysis of buildings. For example, he asserts that, before the 1920s, "apartment" indicated something large and luxurious for the upper class, whereas "tenement" indicated the minimal facilities for the lower class; garden apartments and coops of the 1920s filled in the blank with sizes and costs suited to middle-class budgets and desires. This formulation neglects the thousands of middle-class apartment units built before 1910, especially for a clientele that defined itself as middle class, and points up Plunz's failure to work through the implications of "class" as a category.

Other problems with the book lie in some of the editorial decisions of Columbia University Press. Illustrations are frequently so small that captions cannot be read. Illustration credits are all in the back of the book rather than in the captions, placing unnecessary burdens on the reader whose understanding of Plunz's argument would be enhanced by knowing the sources of his illustrations. There are no page number guides to the text at the top of the endnotes pages, making it far harder than necessary to locate the note one needs.

The book is heavily illustrated with an excellent

selection of period photographs and drawings and architectural plans, sections, elevations, and site plans. The book's virtues include fifty-six pages of notes and bibliography, which will be helpful to students. This book could be a useful text for any history of housing course, because the types of housing that Plunz explores were built beyond the borders of New York City.

ELIZABETH C. CROMLEY  
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Buffalo

RICHARD B. STOTT. *Workers in the Metropolis: Class, Ethnicity, and Youth in Antebellum New York City*. Ithaca: Cornell University Press. 1990. Pp. xiv, 300. \$34.95.

Like the punch lines of the often crude jokes that flavored mid-nineteenth-century New York plebeian life, it is the end of Richard B. Stott's monograph that draws attention. The premise of his study is that in the 1840s and 1850s a "new" working class arose in New York: a class distinct both from a coexisting "middle class" and from an earlier laboring constituency that had developed in New York (as in other northern coastal cities) between the revolution and the late 1830s and that had been dominated by native journeymen and the concerns of artisan republicanism. Tilting gently against a scholarly tendency to comprehend all antebellum urban labor history in terms of this earlier constituency (and the attendant drama of crafts-in-decline), Stott insists that by 1850 the city's labor force had been transformed. Workers were now largely male and young, typically semi- or unskilled, and overwhelmingly immigrant. But most important, as he demonstrates in the book's marvelously engaging last two chapters, laborers now also organized their lives around new (or transformed) institutions and sensibilities.

Stott's run-up to this final discussion is a broadly gauged structural analysis of late antebellum New York's economy and work force. Cogent and solid, this survey depicts a laboring population that, although vulnerable to seasonality and trade depressions and increasingly hunkered down in working-class neighborhoods, generally found jobs, worked hard, and (by European standards) did quite well.

Although scarcely contradicting what has gone before, Scott uses a different tone in his concluding exploration of post-1840 plebeian life outside the workplace. Here he illuminates the boardinghouses, saloons, fire companies, and theaters as pivotal institutions of New York's mid-century working class. He stresses the importance laborers had come to invest in fighting and male physicality and the way urban plebeian politics had shifted from conjurings with republicanism to localistic celebrations of political leaders. And, under the specific rubric of "culture," Stott imaginatively adduces the flowering of distinctive speech patterns among city workers along with an



overall "style" rooted in generosity, cynicism, and defiant protestations of equality. It is a harsh, street- and male-centered ethos Stott evokes, a culture unified less by ideas than by camaraderie, tough humor, and deep resentments. The "new" working class may have been comparatively well-off, but its after-hours culture had rough edges.

One cannot help wishing the book's coda had been its centerpiece. This is less because of problems with Stott's final flourishings about the substance and forms of plebeian culture than because, as flourishings, they leave some issues unresolved. Thus, a less-compressed treatment might have encouraged a more critical handling of the first-person accounts Stott uses so often. It might have permitted him to develop the linkages between workplace and non-workplace cultures as well as to explicate more precisely what "class" meant in a milieu characterized by ethnic and neighborhood divisions. And it might also have facilitated considering topics such as black workers (who are virtually ignored): the possibility that women workers, despite their frequent "supplementary" economic role and the male domination of many working-class discourses, nonetheless achieved cultural space for themselves; or the likelihood that plebeian resentments of this era found some ideological expression in politics and the strikes (which Stott acknowledges but treats fairly cursorily) that sprinkled the 1850s.

But most critical, a fuller treatment of cultural issues might have allowed Stott to situate his "new" working class more firmly in context. Was the break with artisanal traditions really so sharp? And, above all, what was the dynamic between New York's late antebellum laborers and the enveloping notions of bourgeois culture? Stott occasionally concedes interpenetrations of plebeian and nonplebeian sensibilities. But to employ what is ultimately a "cultural definition of class" (p. 273) requires giving central emphasis to the fact that, even as the "new" working class turned inward and contested "genteel values," it was observed by and responsive to bourgeois constituencies; it requires acknowledging that in mid-nineteenth-century New York, culture, like class, rested on relationships.

Yet such admonitions do not shadow the basic strength of Stott's book. That a reader is provoked into probing the limits of this book is evidence of its provocative sparkle. Stott has made a significant contribution by exploring an important contingent of workers at a crucial period.

JONATHAN PRUDE  
Emory University

JAMES L. CROUTHAMEL, *Bennett's New York Herald and the Rise of the Popular Press*. Syracuse: Syracuse University Press. 1989. Pp. xi, 202. \$27.95.

Founded in 1835, the *New York Herald* was not the first of the new cheap mass-circulation urban newspapers launched in that decade, but it quickly became the acknowledged leader and for over three decades reigned as the most successful and widely circulated newspaper in America. Its publisher, James Gordon Bennett, skillfully combined enterprise and efficiency (if not accuracy) in news gathering, sensational detail in presentation, and technological innovation in production. He was, moreover, one of the most controversial, erratic, and unprincipled editorialists in an age noted for its colorful and independent newspapermen. Consequently, anecdotes about Bennett's career and his newspaper's exploits have long been the stuff of journalistic legend. Yet, as James L. Crouthamel notes, there has been no "good, scholarly study" of Bennett or the *Herald*.

Careful scholarship abounds in Crouthamel's book, based on more than twenty-five years of attention to the newspaper, close reading of three decades of the daily *Herald*, and examination of a wide range of relevant manuscript sources. In the absence of personal papers revealing of Bennett, Crouthamel focuses on the public figure, especially as evidenced in his newspaper. In the first three chapters, he describes Bennett's career before he founded the *Herald*, the newspaper's early years, and its exploitation of new technologies in printing and communication to produce what many consider the first modern newspaper in America. Four subsequent chapters, the bulk of the book, provide detailed summaries of Bennett's often-perverse editorial positions on national, state, and local affairs. Although he loudly proclaimed his political independence, Bennett was intensely interested in politics and more often than not promoted the cause of the Democratic party. He was an ardent advocate of American expansion, a violent opponent of abolitionism, an unmitigated racist, and (until Fort Sumter) a consistent defender of the South. His political fancies often wavered, however, as special favors were received or anticipated and as the sentiments of his varied readership demanded. Finally, Crouthamel tentatively assesses Bennett's importance in a short concluding chapter.

Despite the exhaustive research and judicious exposition, this work is disappointing because Crouthamel refuses to engage with the issues that make Bennett's career important. Agreeing with Frederick Merk's assessment of the paper's effectiveness in promoting the doctrine of Manifest Destiny, Crouthamel concludes that readers most likely "bought the *Herald* in spite rather than because of its editorial policy" (p. 161). Yet he pays far less attention to what presumably did attract them, namely, the news that Bennett helped define as a commodity and purveyed in such quantity. Crouthamel rejects several recent interpretations of the rise of the penny press as "long on speculation and short on evidence" (p. 159), but he declines to offer an alternative viewpoint that would help the reader make sense of the rich array of

data that he supplies. He similarly ignores recent scholarship on antebellum America that might, for example, have helped discern patterns among Bennett's prejudices. In short, the book's title is misleading: Crouthamel tells us much about Bennett's *New York Herald* but provides little insight into its role in the rise of the popular press.

SALLY F. GRIFFITH  
Villanova University

GRACE PALLADINO. *Another Civil War: Labor, Capital, and the State in the Anthracite Regions of Pennsylvania, 1840-68*. (The Working Class in American History.) Urbana: University of Illinois Press. 1990. Pp. xii, 195. \$26.50.

What did it mean to resist conscription into the Union Army during the Civil War? Why did opposition to the draft evoke such a repressive response? What were the long-term results of the conflict over conscription? These are the questions that Grace Palladino seeks to answer in her meticulously researched and insightful analysis of the war years in the anthracite coal region of Pennsylvania. By scrutinizing the goals of miners and mine operators, Palladino illuminates the fault lines of a society beginning to fracture along class lines. The conflict and the hysterical response, she contends, had less to do with pro-South sentiments or ethnic clannishness than it did with the ability of mining corporations to use state military force to impose new labor discipline.

The anthracite region's wartime social conflict had its origins in the growing polarization of wealth and political influence in the antebellum years. Although the mine operators' rhetoric emphasized the virtues of the small-scale, individual entrepreneur, by the 1850s large-scale capital dominated the region. Miners were propertyless and dependent on companies that were themselves caught in a market squeeze when prices dropped in the 1850s. Unable to overcome the economic power of mine operators, the concentrations of Irish and (to a lesser extent) German immigrant miners rebuked them at the polls. Rejecting Whig pleas for protective tariffs and nativistic temperance reform, Catholic miners in particular gravitated to the Democrats in a political alliance that was both economic and ethnocultural in character.

The Civil War only added to the contentiousness between labor and capital. Although Democrats as well as Republicans in the anthracite region initially supported the war, emancipation triggered resistance from the political party of the miners. The Democrats opposed both the higher costs of a war to secure emancipation and the potential job competition from freedmen. Furthermore, the war effort demanded increased coal production despite a temporary shortage of labor. This situation provided miners with a perfect opportunity to redress longstanding griev-

ances concerning wages and working conditions, a dreadful prospect to the mine operators.

As Palladino convincingly demonstrates, this is the context in which the draft resistance and the attendant repression need to be understood. Mine operators wanted more from the presence of federal troops than conscription enforcement. Indeed, they even provoked outrage from miners by giving employment lists to authorities that at times resulted in the conscription of ineligible men. When resistance ensued (and mild resistance it was), mine operators wrote alarmed letters to government officials demanding the presence of troops not only to enforce the conscription act but also to suppress "treasonous" labor strife. For capital, then, any opposition to the war effort conveniently became a form of treason that federal authorities could legitimately suppress. Operators saw plotting "Molly Maguires" behind everything that threatened their prosperity and control, a condition that characterized postwar labor relations in the region as well.

This book is an excellent rereading of the opposition to conscription and the rise of labor in the anthracite region. Palladino does not minimize the racism of the miners' opposition to the draft; rather, she explores it in a broader political context. My only wish is that the study could have been extended either geographically or temporally. For students of labor, the Civil War, or ethnicity, however, this volume provides a fresh perspective on the violence in that troubled region.

KEN FONES-WOLF  
West Virginia University

RICHARD GRISWOLD DEL CASTILLO. *The Treaty of Guadalupe Hidalgo: A Legacy of Conflict*. Norman: University of Oklahoma Press. 1990. Pp. xv, 251. \$22.95.

Despite its shortcomings this work adds a new and important dimension to our understanding of the consequences of the Mexican War. In tracing the history of the Treaty of Guadalupe Hidalgo, Richard Griswold del Castillo identifies the many and often-conflicting interpretations given to its provisions, especially articles dealing with citizenship and property rights, by officials, legislatures, commissions, and courts at the local, state, and federal levels in the United States. Chapters on the role of the treaty in the growth of the Chicano movement in the United States since World War II and on its continuing significance in U.S.-Mexican relations are also valuable for understanding the lasting impact of the treaty.

Not surprisingly, the author concludes that the treaty's provisions have frequently been interpreted and applied in ways that violated the letter and spirit of the treaty. He shows that the treaty's framers intended the eighth and ninth articles to protect fully the civil and property rights of the Mexicans and

Indians who became residents of the United States under the terms of the treaty. He chronicles the long and agonizing processes by which these residents have tried to assert their legal rights only to find, in most cases, that they faced administrative and judicial procedures that favored westward expansion and the interests of those who supported expansion. Many of those that the treaty sought to protect lacked an understanding of the judicial system, the financial resources, or the courage necessary to pursue their cases vigorously within the hostile environment that pervaded the area during the century following the war. Even favorable administrative or judicial decision was often overturned by later actions, and the expense of pursuing a property claim to a successful conclusion was often more than the value of the property involved.

Unfortunately, several faults detract from the work's value. The frequent substitution of "America" and "American" for the "United States" in both noun and adjective form suggests an insensitivity unexpected from an author with a Hispanic surname writing on a Chicano topic. Surprising also are inconsistencies in the accenting and citing of Hispanic surnames. Fernando Chacon Gómez is referred to as "Gómez" in the text (p. 145), yet in the bibliography his name is listed as "Chacon Gómez, Fernando," as it is in the index. A similar example is José María Roa Bárcena. In the text (pp. 118–19) and index he is referred to as "Roa Bárcena," while in the bibliography he is listed as "Barcena, José Maria Roa," without accents on Bárcena or María. Although these failings and other discrepancies in source citations can easily be dismissed as an editor's errors, the final responsibility rests with the author.

The work's lack of organizational coherence and integrity is more serious. It appears that several essays on different aspects of the Treaty of Guadalupe Hidalgo were thrown together without the revisions necessary to unify the work and eliminate repetitious material. The five chapters that deal with the causes of the war, treaty negotiations, and historical interpretations of the war and treaty add little to our knowledge and detract from the book's main thesis that the Mexican and Indian populations of the area covered by the treaty have not enjoyed, and still do not enjoy, the full rights and protections apparently given to them by the treaty and guaranteed by the Constitution of the United States.

DONATHON C. OLLIFF  
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GERALD E. POYO. *"With All, and for the Good of All": The Emergence of Popular Nationalism in the Cuban Communities of the United States, 1848–1898*. Durham, N.C.: Duke University Press. 1989. Pp. xvii, 182. \$28.95.

Gerald E. Poyo's approach to the study of the emergence of popular nationalism in the old Cuban com-

munities of Florida in the nineteenth century breaks new ground. Basing his study on the historical collections in the Cuban national archives in Havana, Poyo shows how the Cuban immigrants in the United States had to define themselves vis-à-vis the two major issues in Cuba then: the struggle for independence from Spain and the question of slavery. Cuba's struggle for independence from Spain brought to the fore three political and socioeconomic alternatives against which all Cubans, inside and outside the island, thereafter needed to define themselves: reform as part of the Spanish empire, annexation to the United States, or independence as a republic.

Those who actively fought for independence usually found themselves in exile, and many went into exile precisely to fight for independence, as did José Martí, whose vision for an independent Cuba gives this book its title. Poyo is thus able to trace the development of the nationalist movement for independence among the Cuban communities that existed in the United States after the 1840s, communities that left behind a rich historical record, mostly in the many newspapers that gave voice to their emerging associations and different points of view.

Throughout the book Poyo traces the development of different intellectual currents among different social classes. He shows that, although Cuba's Creole elites were slow to fully embrace a pro-independence nationalism, and in mid-century preferred the option of annexation, as the century wore on they increasingly came to espouse the nationalism that the popular sectors of Cuban society supported. Thus, support for the achievement of independence and republic came to exist throughout all levels of Cuban society.

Support broadened not only as a result of events that took place in the island, such as the Ten Years War, but also as a result of the changing demographic composition of the Cuban community in the United States. Prior to the 1860s, the émigré community had been small and mostly white and middle class; by the mid-1870s, the community had expanded substantially and comprised both black and white Cubans as well as distinctly working-class communities. The new immigrants were not only "pushed" by the fear engendered by the Spanish response to the struggle for independence but also "pulled" by the new opportunities created by the new cigar industry in Tampa and Key West. These tobacco workers brought with them strong patriotic sentiments that provided the Key West nationalist leaders with a political constituency. In turn, the exiles' nationalist movement increasingly exerted influence on the course of the Cuban struggle for independence on the island.

The Cuban tobacco workers became the movement's primary constituency as they enthusiastically embraced the nationalism represented by the leaders of the independence movement in Cuba: Máximo Gómez, Antonio Maceo, and especially José Martí, who lived and wrote among them, in exile. At the same time, Cuban tobacco workers felt conflict over

whether to place their energies and loyalties behind labor or nationalist organizing, differences that divided the Key West and Tampa communities. That rift was further exacerbated by the issue of race, as black Cubans distrusted the continuing racism of nationalist leaders and pressed for the abolition of slavery along with independence. The encompassing and egalitarian vision of Martí's nationalism gained the support of both black Cubans and workers, who dedicated themselves to raising funds for the Cuban army's insurrection on the island. In the end, the independence that Cuba achieved was not that envisioned by Martí but a compromised sovereignty resulting from U.S. intervention to end the war quickly, intervention that neutralized the Cuban popular nationalist movement.

This book is must reading for every recent Cuban exile who thinks that Cuban-American history dates to the Cuban revolution; for every Hispanic American who thinks that Cubans are too recently arrived to have a history, properly speaking; for every scholar of immigration who thinks that the questions immigrants pose pertain only to their assimilation; and for every scholar of Latin America who thinks that Latin American history ends at the border with the United States.

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JAMES MARTEN. *Texas Divided: Loyalty and Dissent in the Lone Star State, 1856-1874*. Lexington: University Press of Kentucky. 1990. Pp. x, 246. \$25.00.

In this work, James Marten undertakes to chart the paths of "loyalty" and "dissent" in Texas over an eighteen-year span. He treats a familiar period but from a different perspective.

Marten demonstrates that Texas was a somewhat reluctant member of the Confederacy. Governor Sam Houston resigned rather than take an oath of allegiance to the government established at Montgomery, and one in four Texans voted against secession. The author believes that Texas blacks and Hispanics were generally passive to the conflict swirling around them, and a large number of Germans opposed slavery and thus disunion. After having fought for ten years to gain statehood and thus the protection of the federal army on the frontier, many Texans retained their loyalty to the Union.

The war years also resulted in attitudes of ambivalence. Although James W. Throckmorton was one of only six legislators who voted against secession, he fought with the Confederacy. Often a "Unionist" in Texas meant agreement with Governor Houston in his defense of slavery but abhorrence of secession. Marten is certainly correct in observing that annexation bred a fidelity to the Union that some found difficult to cast off. Within the state, however, there

were strong pressures to conform to the southern cause. Suspected Unionists were tormented, harassed, and even lynched. Perhaps the worst such episode occurred at Gainesville in northern Texas in October 1862. There forty-two men, falsely suspected of being pro-Union, were hanged, some without benefit of a judicial proceeding of any kind. Resentment against the antislavery sentiments of Texas Germans and Hispanics also led to repeated acts of violence directed at those two groups.

The author believes that the divided feelings that shaped Texas history to the Civil War continued beyond it. Five men, representing every spectrum of Texas politics, governed during the Reconstruction period. The aforementioned Throckmorton hung portraits of Robert E. Lee and Jefferson Davis in the state capitol. Elisha M. Pease, a prewar governor and loyal to the United States throughout, returned briefly as a Republican governor committed to black emancipation. Andrew Jackson Hamilton and Edmund J. Davis sought to implement Radical Reconstruction with the enthusiastic military support of the Grant administration. Finally, Richard Coke, a Confederate veteran, returned the state to the local Democratic party in 1873. This period also witnessed the rise of the Ku Klux Klan and similar groups designed to stifle dissent of any kind. Glorification of the Confederacy and a determination to maintain white supremacy became the ruling passions in Texas.

Marten has written an admirable book. His research is extensive, and he employs a graceful and fluid writing style. This study enriches our knowledge of Texas history.

STANLEY E. SIEGEL  
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J. MATTHEW GALLMAN. *Mastering Wartime: A Social History of Philadelphia during the Civil War*. (Interdisciplinary Perspectives on Modern History.) New York: Cambridge University Press. 1990. Pp. xiv, 354. \$49.50.

Historians have paid scant attention to the impact of the Civil War on American society, and J. Matthew Gallman seeks to redress this imbalance. In this book he attempts to evaluate the influence of the war on aspects of public and private life in Philadelphia: army recruiting, ties among kin, civic celebration, philanthropy, public order and crime, labor organization and relations, and economic growth and opportunity. Gallman's conclusion is the same in each domain, namely, that Philadelphia was remarkably successful in meeting the challenges of the war because of the resilience of its antebellum institutions, which survived the war with modification but no wholesale change. Continuity is the stanza and refrain of this book.

Urban voluntary associations—church groups,



philanthropic and fraternal societies, business and trades networks, and fire companies, among others—are Gallman's *dramatis personae*. The vitality of these associations, he argues, assured that authority would remain local and decentralized in wartime Philadelphia, despite the Republican party's creation of a vast federal apparatus to manage the war effort and despite the problems of mass mobilization associated with what Allan Nevins called "the organized war." Part of Philadelphia's wartime "success" can be seen in the relative absence of public disorder. Gallman attributes the lack of New York-style mob violence to the prewar strengthening of the police force, which allowed the shrewd mayor Alexander Henry to anticipate and prevent the flare-up of rioting. The 1870s, and not the war decade, are Gallman's watershed of social change; in the 1870s, centralization, defined as "expanding initiatives by public officials," became increasingly evident (p. 340).

This extensively researched book persuades the reader of its central claim that Philadelphians relied on familiar antebellum institutions and traditions to respond to the challenges of the war. One of Gallman's accomplishments is to demonstrate the extent to which the activities of the "state" during the Civil War depended on a blend of public and private initiatives: the federal provost guard, responsible for disciplining the sometimes unruly soldiers that passed through the city, hired civilian guards and agents; the U.S. Sanitary Commission was a quasi-public national organization that gained official recognition, but its great Philadelphia fair of 1864 relied heavily on the city's preexisting voluntary associations and private networks for funding and organization. This blurring of the boundary between public and private authority (which renders anachronistic much of the current debate over the state—bring it in or leave it out—when applied to the nineteenth century) merits more analysis from Gallman as a phenomenon in its own right, not merely, as he treats it, as evidence of the persistence of private initiative. Nonetheless, this book provides an abundance of illuminating detail concerning the ways authority was constructed in a Civil War-era city.

It would be useful to know more of the alternative paths of development Philadelphia might have taken and what substantial "change" would have looked like—surely we should not expect in 1863 to find a World War I-style Committee on Public Information or War Industries Board setting up shop on Walnut Street. It also seems hard to account for Philadelphia's stability without entering more fully into the realms of culture and ideology. The Philadelphia elite (in this regard, so unlike its New York counterpart) found it relatively easy to act in concert, accommodate dissenting voices, and anticipate threats to public order. One would like to know more about the origins of this accommodating style of leadership and the flexible style of entrepreneurship that allowed Philadelphia businessmen to adjust so well to the economic

crises and opportunities of the war. Party politics, which Gallman places outside the scope of his analysis, also seems relevant to the issue of stability—the freedom that Lincoln gave to Philadelphia to develop its own responses to the war was doubtless related to the 52 percent of votes he received from Philadelphians in the election of 1860. These points should not obscure the fact that this significant book is necessary reading for those who wish to understand the construction of public order and the transformation of urban institutions in the nineteenth century.

IVER BERNSTEIN  
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MARY A. DECREDICO. *Patriotism for Profit: Georgia's Urban Entrepreneurs and the Confederate War Effort*. (The Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1990. Pp. xx, 211. \$29.95.

When the Civil War erupted, the nascent Confederacy had only two states with economies sophisticated enough to include not only commercial agriculture but also major urban, industrial, and railroad developments. Virginia, on the periphery, quickly felt the Union onslaught. Deep in the rebel interior, Georgia rendered essential services until well into 1864, when William T. Sherman's army finally brought the real war to the Empire State of the South.

A diversified, expanding economy had emerged in antebellum Georgia. Fast growing railroads brought prosperity to the old urban centers of Savannah and Augusta and the new interior cities of Macon, Atlanta, and Columbus. Industrialization, especially textile manufacturing, also developed steadily as the so-called New South began to appear in old Georgia. A number of modern businessmen and entrepreneurs spearheaded this surge with considerable encouragement from local and state governments.

When the war came, these men, mostly native southerners, optimistically led a rapid economic expansion as new Confederate facilities and more and more Confederate contracts increasingly dominated Georgia's mushrooming military-industrial complex. But, even at the height of this wartime boom, shortages of workers and materials and persistent inflation gave warnings of the perils of waging modern war against the North and its overwhelming resources. Confederate mismanagement, especially of the extensive but fragile rail network, also hurt, but the decisive blow was Sherman's invasion, which shattered the economy (and the morale) of Georgia and really broke the back of the whole Confederacy.

The southern nation died, but some of the same businessmen-entrepreneurs-managers who had performed so impressively before and during the war stepped forward again to rebuild quickly Georgia's diversified economy and lay the foundation for the

vaunted New South whose taproot ran back straight and true through the Civil War to the late antebellum period.

Mary A. DeCredico has written a unique and quite effective monograph. Focusing on Savannah, Augusta, Atlanta, and Columbus, she brings out of the shadows some of the big and a few of the little businessmen who made the urban-industrial sector of Georgia's complex economy work in peace and in war and then in peace again. Many performed efficiently, making money and also serving the needs of the state, and William M. Wadley, president of the Central of Georgia Railroad (and a transplanted Yankee), emerges as a real hero, quite possibly worthy of a separate study.

In this volume DeCredico could have said a little more about broader policies in Georgia and the Confederacy, and she should have said a lot more about Macon, the state's only other real city at that time. Overall, however, she has produced a solid study that demonstrates continuity during a period of great upheaval. Indeed, this book could well serve as a model for studies of other Confederate states with urban centers that struggled to develop within an agrarian environment.

F. N. BONEY  
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DON H. DOYLE. *New Men, New Cities, New South: Atlanta, Nashville, Charleston, Mobile, 1860–1910*. (The Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1990. Pp. xix, 369. Cloth \$39.95, paper \$12.95.

Don D. Doyle's comparative study of Atlanta, Nashville, Charleston, and Mobile between 1860 and 1910 traces the rise of a regional urban network and an urban elite as they helped define the New South. In choosing these cities, the author examines the New South at its strongest (Atlanta) and weakest (Charleston) points and a couple of "places in between." Early chapters sketch the impact of the Civil War and Reconstruction, the region's postwar economic transformation, and the appearance of an urban network in which newly ascendant interior cities capitalized on developing railroad connections. Atlanta and Nashville thus proved most dynamic, whereas Charleston and Mobile "sank into a prolonged season of demoralizing stagnation" (p. 51).

The core of Doyle's well-researched book involves the question of human agency in the city-building process. The architects of the new order were the emergent merchants, financiers, industrialists, and their allies, particularly those in the press. Generally settled into middle age and longtime residents of their respective cities, they were overwhelmingly southern, although recent northern arrivals made their influence felt. Doyle acknowledges the latter while emphasizing the importance of the former

largely because of their numerical preponderance—perhaps an arguable standard.

The new business class flourished in Atlanta and Nashville, making an appearance in the laggard seaports only in the 1890s. It was characterized (in varying degrees) by ardent entrepreneurship, relentless efforts at organization, and a new-found unity born of the willingness to sacrifice for the "common welfare" (p. 159). Increasingly sources of social and economic power, the new elites reified their identity through a welter of new social institutions that transformed wives and daughters into "gatekeepers" entrusted with defining standards of propriety. Linking their own well-being to economic development, social reform, and investments in "human capital," the author argues, the champions of the New South "failed to undermine completely the foundations of the old regime" but successfully "open[ed] the road to the modern South" (p. 318).

Forcefully argued, Doyle's work nevertheless presents some problems. First, the author takes too much of the business rhetoric at face value. The claim that city boosting "subordinated the pursuit of self-aggrandizement" (p. 136), for example, seems questionable. If it became necessary to pursue self-interest collectively through new organizations, the New South's business class showed little of the implied spirit of self-sacrifice. As Doyle notes in Atlanta's struggle to extend vital services, the boosters' concern often extended no further than "the mercantile portion of the city" (p. 141).

The most problematic part of the argument, however, revolves around the question of race. There is little discussion of the new elite's relationship to the white working class, and that omission weakens the analysis of the new racial order. Doyle also places the most benign construction on elite behavior, stressing their "commitment" to racial uplift and "optimism" regarding the possibility of black "progress." If the leadership's practice belied its rhetoric, Doyle blames externally imposed limitations. That formulation fails to recognize that the rejection of the old paternalism merely relieved business leaders of individual responsibility in racial affairs even as they refused to assume any substantive social responsibility. They were, in short, devoted as much to white supremacy as economic modernization; if any compromises were forced in the pursuit of either, they usually came in the latter rather than the former. In short, segregation and the deplorable condition of black life represented less the business class's "failure . . . to transcend the region's traditional system of racial caste" (p. 288), as Doyle contends, and more its "success" in adapting it to new realities as suggested by John Cell in *The Highest Stage of White Supremacy* (1982), a work mentioned only in a single footnote. The difference is important, for, far from trying to "undermine" the racial mores of the old regime, the emergent urban elite sought nothing more than a new industrial society consonant with them. They were, indeed, as

the rest of Doyle's argument contends, the architects of the new order and not thwarted subversives.

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MICHAEL L. LANZA. *Agrarianism and Reconstruction Politics: The Southern Homestead Act*. Baton Rouge: Louisiana State University Press. 1990. Pp. x, 153. \$22.50.

In 1866, the "critical year" of Reconstruction, Congress enacted the Southern Homestead Act. Modeled after the landmark Homestead law of 1862, it was an attempt to bring the agrarian myth to southern blacks. In Alabama, Arkansas, Florida, Louisiana, and Mississippi—the southern public land states—were forty-six million acres of federal land. The new law allowed any person, without distinction of color, to claim 80 acres (later 160) of this domain. A homesteader had to make improvements, occupy the land for five years, and pay a five-dollar fee to obtain final title. The measure's Republican creators left no doubt that they intended freedmen to be the principal beneficiaries. "The object of this bill is to cut the public land up into small homesteads," said Republican Senator Samuel Pomeroy, "and it need not be disguised that it is aimed particularly for the benefit of the colored man, those who have not been able hitherto to acquire homesteads on the public domain in these States" (p. 21).

Michael L. Lanza's book is the disturbing story of the Southern Homestead Act. So strong was their faith in the family farm and their own free labor ideology that Republican leaders paid scant attention to land quality. They equated mere possession of the soil with successful farming, ignoring the fact that the public land in the South was unclaimed because it was generally unfit for cultivation. Most of the land was not surveyed, and the existing land records were a mess. For many homesteaders, simply finding the exact location of a tract was a major obstacle. Many people filed on land that turned out to be reserved for public use or already claimed by someone else. If all this was not enough, there were unscrupulous timber companies and corrupt, ignorant, incompetent, and racist public officials who either did not understand the law or deliberately misconstrued it. In the end, "the homestead act provided nothing to those who had nothing" (p. 87).

The verdict that the law was a failure is a familiar one. Lanza's contribution is to examine what went wrong in more depth than have previous historians. Perhaps, though, the emphasis on the law's failure is overdone. True, it was repealed in 1876, and a majority of the homesteaders gave up. On the other hand, 41 percent of the settlers, a sizable minority of whom were black, actually stuck it out and won title to their land. Even under a more realistic and benign system, one would have expected a high rate of failure from the least-privileged members, black or

white, of southern society. Even if the figure of 41 percent is somewhat inflated, as Lanza suggests, it nonetheless strikes me as impressive. Southern homesteaders won title to three and one-half times as much acreage as the federal government held at the end of the Civil War under the confiscation and abandoned property acts. We learn a great deal in this book about why the majority failed but very little about how this significant minority achieved a modicum of success. Their story is worth exploring.

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SHERRY L. SMITH. *The View from Officers' Row: Army Perceptions of Western Indians*. Tucson: University of Arizona Press. 1990. Pp. xix, 263. \$24.95.

Sherry L. Smith surveys army officers' ideas about western American Indians from 1848 to 1890. Using army records, diaries, and letters of frontier soldiers and their wives, Smith has compiled a fascinating work that reveals much about white values in the late nineteenth-century United States.

Smith amply illustrates that, although there were indeed prejudices among military officers against the western Indians, not all officers held the same adverse opinions about the tribes. There were "various shades of gray" between the images of "good" and "bad" Indians (p. 185). Officers had ambivalent feelings about the tribes, and most appear to have agreed with Colonel Richard Irving Dodge's assessment that, although Indians were "swindled, starved, and imposed upon," they were also "savages" and had mental capacities "estimated at zero" (pp. 20, 29). Many officers who fought fiercely against them still felt sympathetic to the Indians' situations, and some officers even openly denounced government officials for dealing unfairly with the tribes. But the officers still fought out of loyalty to the U.S. government and to their beliefs that Indians would never advance to the higher stages of civilization and that whites could better use Indian lands.

The chapter "Tribes and Chiefs" reveals that many officers could make differentiations between tribes. Evaluating tribesmen by their fighting abilities, officers admired the Cheyennes ("formidable in appearance"), the Nez Perce ("had an equal love of fighting and [Christian] devotion"), the Sioux ("finest physical specimens"), and the Apaches ("fiercer than tigers") over the more "effeminate" tribesmen of the Northwest coast. Chiefs and leaders were considered to be the most "normal" and dignified tribal members (p. 47).

Officers were also divided in their opinions on federal Indian policy. Some believed that the best plan was to give Indians western lands, whereas others wanted to confine them to reservations. Extinction was a popular alternative, but many men were adamant about assimilating Indians into white

society and pointed to the "Civilized Tribes" of eastern Indian Territory as examples.

The interesting chapter on Indian women shows that military men viewed tribal females in one of two ways. Either they were homely and subservient "squaws," who sometimes engaged in battle (the image of Indian females as fighters helped justify the killing of women), or they were seen as noble and beautiful "Indian Princesses," who were inclined to assimilate and to marry white men. Officers expressed their admiration for the Indian women's active lifestyles, contrasting them with white females' "do nothing, thankless, dyspeptic" sphere. The officers' wives, on the other hand, never described the Indian women as having a better station than they did.

Because the officers judged Indians according to the values of white society, their opinions reveal more about white America than about the actual tribal cultures. What is needed now is a comparable work on Indians' viewpoints of white Americans.

DEVON A. MIHESUAH

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JANET ROBERTSON. *The Magnificent Mountain Women: Adventures in the Colorado Rockies*. Lincoln: University of Nebraska Press. 1990. Pp. xxi, 220. \$21.95.

As part of the effort to redefine given truths about women's experience in the past, Janet Robertson has collected the stories of women who chose the strenuous life in Colorado's Front Range. Beginning with pioneer Julia Archibald Holmes, who in 1858 was the first white woman to climb Pikes Peak, Robertson not only chronicles women's first ascents but also includes the contributions of women botanists, homesteaders, conservationists, and trail builders and describes the skill of women rock climbers and "peak baggers" of Colorado's fifty-four "fourteeners," as its mountains over fourteen thousand feet are called.

Although the personal reasons for their activities varied, most of the women were originally from the East, college educated, white, and middle class. They were looking for personal challenges and direct experiences with nature. In 1873 suffrage lecturer Anna Dickinson climbed Longs Peak with members of the Hayden survey team in the name of woman's rights. She named an adjoining peak Lady Washington as a companion to the highest of the New England presidentials. Harriet Vaille interviewed Arapaho elders in order to restore Indian place names in the proposed Rocky Mountain National Park. Virginia Donaghe McClurg devoted her life to preserving the cliff dwellings at Mesa Verda, although her goal of keeping it as a woman's park was later eclipsed. Dr. Susan Anderson traveled over Berthoud Pass to the Fraser Valley to practice medicine after discovering that lumberjacks accepted a woman doctor more readily than did patients in Denver. Anna

Gudrun Gaskill, in 1977 the first woman to be elected president of the Colorado Mountain Club, was the force behind the creation of the Colorado Mountain Trail.

Although Robertson gives us some clues, the significance of how these women and their activities were viewed by the broader society could be explored further. For the earliest among them—botanist-explorer Alice Eastwood, skier Marjorie Perry, climber Eleanor Davis, and the women of the new Colorado Mountain Club—what they wore was just as important to the public as what they did. As adventurous as all of them were, they did not dare give up their skirts until a few years before the 1920s. They invented all kinds of devices with buttons, flaps, and tunics to make their activities possible, masking the real meaning of their achievements at a time when conservatives believed women's rights and education would unsex women and lead to "race suicide." With the exception of Mount Dickinson, few physical features in Colorado (and elsewhere) carry women's names, and usually when they do, only the first name is used. When male geographers assign women's first names to lakes and men's last names to mountains, the sexual symbolism is clear. Both the assumptions about women's clothing and the naming traditions reveal the attitude that a woman could climb mountains, literally and figuratively, as long as she did not neglect her primary role.

Colorado was the second state to grant women suffrage (1893) with a goal of attracting women settlers. It would be interesting to know if any of these pioneering women moved to Colorado because they believed they would have more rights. The effect of the modern women's movement on Colorado's "mountain women" is striking. Until the 1970s, Robertson tells us, most women were accompanied by men on their mountaineering expeditions. A symbolic switch came in 1982 when Coral Bowman Wilber opened her Great Horizons Climbing School to men.

This book is a lively account of women seeking and achieving self-reliance in the natural world, enhanced by a fine collection of period photographs. It is a refreshing addition to the history of the outdoors, sports, and women in the American West.

POLLY WELTS KAUFMAN

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PEGGY PASCOE. *Relations of Rescue: The Search for Female Moral Authority in the American West, 1874–1939*. New York: Oxford University Press. 1990. Pp. xxiii, 301. \$29.95.

Peggy Pascoe's brilliant analysis of Protestant women's home mission organizations is indispensable reading for any historian of late nineteenth- or early twentieth-century America. Organized and written



with verve and clarity, it is based on institutional records from four western examples—a Chinese rescue mission in San Francisco, a Denver home for unwed mothers, a Salt Lake City refuge for unhappy wives of Mormon polygamists, and a program for Omaha Reservation Indians. Middle-class Protestant women of this era transformed Victorian belief in female piety and purity into a tool of emancipation, a justification for defying patriarchal behavior and rescuing female victims from what they saw as “male lust and dominance,” says Pascoe. “Women together could make a home, while a woman and a man ‘of doubtful character’ could not, because patriarchal behavior was a primary threat to Protestant women’s conception of the Christian home” (p. 39).

This “search for female moral authority” fueled significant anger toward the male-dominated social order that initially opposed most of the missions. The anger was exacerbated by conditions in western cities, Pascoe says, making it a notable phenomenon of western community development. (I think more eastern examples might prove the conditions less regional, such as vitriolic accusations of “baby farming” by Connecticut officials when Virginia Thrall Smith incorporated adoption programs into her Hartford City Mission.)

Though infused with the “racialism” of their time and convinced of their Protestant superiority, mission women did challenge biological determinism, insisting on the equal potential of all women and often providing the means for them to achieve it. Pascoe finds the interpretations of both liberal and radical historians inadequate. These women were not just selfless reformers choosing to help the downtrodden, nor were they rigid middle-class Victorians imposing social control on the disadvantaged. In fact, their emphasis on education and professionalism for women, elimination of the sexual double standard, and the exercise of more female influence appealed to many women who chose their help. Prostitutes, abused wives, and abandoned pregnant women used rescue homes as a means of escape and a new beginning, even if they rejected the mission ideology.

Twentieth-century changes brought about the demise of most mission projects. Fund-raising shifted from local to national organizations, government and legal cooperation replaced earlier opposition, and “scientific charity” in a social work bureaucracy led to state regulation and loss of autonomy. There was also a transformation of the gender system, a decline in Protestant evangelicalism, and a rise in cultural relativism—all of which undermined the rationale of home missions.

Pascoe suggests similarities between Victorian missionaries and modern “cultural feminists,” both tending to define “woman’s culture” according to middle-class values and supposedly universal female experience, sometimes overlooking ethnic or individual differences, and branding women who disagree as immoral. As Pascoe puts it, “To the extent that

focusing on women makes men peripheral to the analysis, it hampers the creation of effective strategies for dislodging the male-dominated power structures that affect most women’s lives” (p. 211). Her book shows clearly why and how this can happen.

RUTH BARNES MOYNIHAN

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LISA M. FINE. *The Souls of the Skyscraper: Female Clerical Workers in Chicago, 1870–1930*. (Women in the Political Economy.) Philadelphia: Temple University Press. 1990. Pp. xx, 249. \$34.95.

Lisa M. Fine has written the history of the development and expansion of the clerical work force in Chicago from 1870 to 1930. During the period, census data reveal that the size of the clerical work force grew from a few hundred workers to almost a quarter million. Fine describes the technological and organizational innovations that led to the rapid expansion of clerical work. The invention of the typewriter and the new position of stenographer-typist transformed the field from an overwhelmingly male preserve to one that was about 50 percent female by the 1920s.

Fine’s chief concern is with the “feminization” of clerical work, and, accordingly, she focuses on the lives of the “white, young, single, and native born” women (p. xvii) who took the new female clerical positions. She examines the division of labor in the office. She traces the growth of the educational, residential, and community institutions that were created by or for the new female clerical workers. Finally, Fine explores the ideological debates about the propriety of women entering clerical work and the gender of the field as a whole.

The book adds to a growing literature on the history of women’s work and thus fills in our understanding of the organizational, social, and ideological changes that took place both at the workplace and within the urban community as respectable women left the domestic sphere to go out to work in the late nineteenth and early twentieth century. Fine shows how the pioneer generation of women clerical workers were depicted as “conquering” a male realm (p. 29). Women entered clerical work primarily in the new occupation of stenographer-typist. Their presence required new standards of office behavior for both men and women and new notions of how young women should integrate their working lives with their future roles as wives and mothers. Fine shows that contemporary commentators were especially concerned that female clerical workers would be preyed on by male bosses and coworkers or would, as Jane Addams put it, “fall into a vicious life from the sheer lack of social restraint” (p. 58).

Working out answers to all of these new issues—from workplace rules to pay scales, to office etiquette, and to codes of behavior for the clerical worker—took

place in the early twentieth century. By the 1920s, Fine argues, the female office worker was "conventionalized" (p. 167). She still held a relatively privileged position—in terms of wages, working conditions, and status—in the context of a highly segregated female occupational order, but Fine argues that clerical work was devalued and further segregated as it was feminized.

Yet, even in 1930, Fine's evidence indicates that almost half of Chicago's clerical work force was male. The process of feminization was not yet complete. That phase of the story—which took place after World War II—is, perhaps, the story of departure of the men in the field, the arrival of the married woman clerical worker, and the opening up of the field to minority women. The later period still awaits the kind of in-depth analysis Fine has given the 1870–1930 period.

MARGO ANDERSON  
University of Wisconsin,  
Milwaukee

CYNTHIA GRANT TUCKER. *Prophetic Sisterhood: Liberal Women Ministers of the Frontier, 1880–1930*. Boston: Beacon. 1990. Pp. xii, 298. \$24.95.

In the quest for a usable past, female religious figures are a much sought after commodity. Cynthia Tucker has recovered an appealing group of potential role models. Her book provides an intimate group biography of twenty women who held pulpits in liberal (Unitarian or Universalist) churches in the Midwest during the last decades of the nineteenth century. This group was remarkable in a variety of ways: they assumed pulpits at a time when women in ministry were virtually unknown; they planted new churches that prospered under their leadership; they formed long-term partnerships among themselves in order to fill the responsibilities both of the pastor and the pastor's wife; and they trained younger women to join what became a network of women ministers. The group defended their right to their pulpits against the more conservative policies of the eastern establishments in their churches. They were united by the fight for woman suffrage and divided about the best way to achieve peace in World War I.

This "spiritual sisterhood" undertook a "domestic ministry" that played on the overlap between liberal theology and maternal values. One minister, Florence Buck, received an especially warm reception for a funeral sermon for a member of the Bartenders' Union. "The next day, the word went round that the bartenders, fully expecting to be sent to hell, had been amazed that the preacher had instead talked to them just 'like a mother.' 'It was an opportunity that I would not have given up to preach before the President and Senate of the United States,' Buck told her sisters" (p. 70).

Stories like this one will make this book satisfying

reading for those interested in the individual experience of feminist foremothers, but readers concerned with the broader historical significance of that experience may be disappointed. For the latter group, Tucker's account of this fascinating group of women will raise a number of questions. Did the message preached by these women differ from that of their brother ministers? Why did women flourish as leaders in liberal pulpits in the Midwest when the Unitarian establishment in Boston was unreceptive to women ministers? The author makes recourse to a frontier thesis: the democratic individualism of the frontier enabled women to assume roles from which they were barred in elitist and hierarchical eastern circles. Indeed, liberals may have been shorthanded in the small towns of the Midwest—but does this justify applying a frontier thesis to turn-of-the-century women who served terms in Chicago settlement houses, showing the hallmarks of progressive reformers? Tucker views the demise of the sisterhood in the first decades of the twentieth century as part of a Unitarian and Universalist reaction against the "feminization" of religion. Although slim on interpretation, Tucker's thorough research in personal papers, church records, and periodicals makes a welcome contribution by documenting the life and work of twenty little-known women ministers.

ANN BRAUDE  
Carleton College

ANDREW R. HEINZE. *Adapting to Abundance: Jewish Immigrants, Mass Consumption, and the Search for American Identity*. (Columbia History of Urban Life.) New York: Columbia University Press. 1990. Pp. x, 276. \$35.00.

Andrew R. Heinze has crammed an enormous amount of information—some of it familiar, some strikingly new—into his book. The familiar parts appear in the background content that he supplies at the start of each new topic; the new content—some of it factual, most of it interpretive—follows. His central idea is that consumerism reflects culture. What people spend their money on tells us what they are or want to be. In the case of the Eastern European Jewish immigrants, he says that their consumption patterns reflected their desire and ability to become "Americans" as quickly as possible.

But, and this is one of his interesting interpretations, they did not "throw out the baby with the bath water" but rather adapted their customs to the habits and holidays of the majority of Americans. The changes took place on a two-way street. At the same time that the Jews were changing their consumption habits, they were also changing the face of at least two commercial occupations ("street marketing and movie marketing"), encouraging the growth of brand-name advertising, and transforming a sleepy rural area in the Catskill Mountains of New York into an "earthly paradise" for their vacations (p. 5).

The book is enriched by more than a dozen excellent illustrations, including an advertisement in Yiddish for Uneeda Biscuits. It is also written in a pleasant style. A work that presents interesting interpretations and informative pictures and is quite readable would seem to add up to a very good book indeed, but, unfortunately, there are a number of problems with Heinze's work that make it difficult to give the volume unqualified approval. The most serious fault is his failure to allow for the fact that the majority of Eastern European Jews came to the United States over a fifty-year period (1870–1920). At any given moment, some behaved as the prosperous consumers and entrepreneurs that he describes, but thousands of others were struggling to establish themselves and were unable to attempt assimilation via consumerism.

Other faults in the book can be summed up by the prefix "over." Heinze overstates and overgeneralizes. For example, he says that, "because of an overriding desire to become established in the United States, Eastern European Jews responded especially quickly to the conditions of mass consumption. They recognized that, as consumers, they could begin to move toward the goal of fitting into American society" (p. 35). Let us be realistic. All people want a comfortable life and will be active consumers when they can afford to be. The aura Heinze has placed around Eastern European Jews is misplaced. Another example of overstatement relates to the slackened interest of Jewish immigrants in their holy days, which he accounts for by saying that, to the newcomers in America surrounded by all of the good things they could now afford, every day was a holiday! This overlooks the host of other forces that led to a decline in religious observance among American Jews.

Heinze has presented an interesting theme. Consumerism is a reflection of culture for all people. He has, however, gone astray by trying to extract too much from his theme. Jews, like other immigrants, traveled many different roads toward assimilation. It would seem that the use they made of free public education in New York, for example, is at least as strong an indication of their desire to join the mainstream and deserves to be discussed in a book that deals with the Jewish "search for American identity."

SELMA BERROL  
Baruch College,  
City University of New York

JAMES HARVEY YOUNG. *Pure Food: Securing the Federal Food and Drugs Act of 1906*. Princeton: Princeton University Press. 1989. Pp. xiii, 312. \$29.95.

This book explores an important and neglected part of the American public health record. Concerned with the struggle for public regulation of the manufacture and sale of food and drugs, James Harvey Young touches on regulation in the colonial era,

surveys the climate of control in the first half of the nineteenth century, but deals mostly with the period from 1879, when the first bill regulating food and drugs appeared in Congress, to 1906, when Congress enacted such legislation.

Young shows that most Americans of the early nineteenth century seemed largely unconcerned with matters affecting public health. State legislatures showed little interest in combating public health abuses, and most had even repealed their medical practice laws. Yet, as Young explains, notable steps were taken in the direction of reform. The general indifference of the press was offset occasionally by some crusading journal decrying the filth of slaughtering houses and milk establishments and the widespread consumption of swill milk, largely bereft of nutrients, produced by cows consuming brewery wastes. Furthermore, the appearance of the *United States Pharmacopoeia* in 1820 demonstrated that some notable pharmacists and doctors had joined to offer a drug standard to reduce prescription difficulties. Concern for public health was further advanced when Lewis Caleb Beck turned from publishing his chemistry text in 1831 to studying food adulteration, which he described in an influential treatise fifteen years later. The major political advance came in 1848 when Congress, attempting to provide pure drugs for American forces in the Mexican War, passed the Drug Importation Act to stop the flow of adulterated drugs from abroad. Even though the enforcement of the measure largely lapsed in less than a decade, it set a precedent for federal involvement in drug control.

Reconstruction politics gave way to other issues as the 1880s approached, and Young describes the rising interest in public health reform. Such fiery crusaders as George T. Angell publicized the extent of the adulteration problem. The Board of Health, which Congress created in 1879, gave cautious support to the agitation, and, in the Department of Agriculture, Harvey W. Wiley turned the Division of Chemistry toward research on food adulteration. The oleomargarine controversy illustrates how a food issue could touch off volatile exchanges in Congress. The controversy temporarily subsided in 1886 with a levy of a tax of two cents per pound on manufacturers of the "greasy counterfeit" but only after representatives from the northern dairy states jeopardized the Democratic party's search for harmony by their attacks on southern spokesmen for the product. Proponents of general food regulatory legislation got no more from Congress in the next decade than Senate passage of the Paddock bill in 1892.

Young's closest concentration is on the politics of health and medicine in the Progressive era. He shows a growing public interest in health affairs after the "embalmed beef" scandal (which he discredits) arose during the war with Spain and following federal investigations by a committee headed by General Grenville M. Dodge and a court of inquiry headed by Major General James F. Wade. Senator William E.

Mason's hearings on adulteration and Wiley's widely publicized "Poison Squad" helped to sustain public interest, which peaked after publication of Upton Sinclair's *The Jungle* in 1905. Young observes that society was coming to believe that only the federal government could appropriate adequately the products of a growing scientific technology for public benefit. He accepts the view that organized middle-class businessmen who deplored the chaos of the economic scene supported federal intervention to achieve stability, efficiency, and order. He identifies components of the coalition Wiley guided in support of food and drugs legislation as well as the opposition that included not only powerful corporate elements but also a dwindling component of state health officials who feared that federal control would threaten their power. His analysis of the discord within both coalitions is especially enlightening.

Young provides a meticulous account of the congressional struggle over the passage of food and drugs legislation and President Theodore Roosevelt's part in the fray. He gives a thorough analysis of the Pure Food and Drugs Act and the Meat Inspection Amendment and, in a final chapter, explores the scholarly interpretations offered since the late 1920s to explain the pure food and drugs struggle. He believes that no single interpretation will suffice.

This is truly an excellent book that should be ranked as the most authoritative and definitive work on the nation's struggle for food and drugs legislation through passage of the basic laws of 1906. Perhaps the greatest of its many strengths is its thorough integration of scientific with political and social history. It is a product of some four decades of research on food and drugs issues by one whose expertise in this area is unsurpassed.

JAMES G. BURROW  
*Abilene Christian University*

JONGSUK CHAY. *Diplomacy of Asymmetry: Korean-American Relations to 1910*. Honolulu: University of Hawaii Press. 1990. Pp. x, 239. \$32.00.

"To the Americans, Korea was insignificant; but to the Koreans, the United States was very important in every respect" (p. 16). This asymmetry explains the unhappy course of Korean-U.S. interaction during the half-century before formal Japanese annexation of the peninsula in 1910, Jongsuk Chay argues, and it left a bittersweet legacy for Koreans and Americans in the twentieth century. Chay identifies seven factors that shaped the relationship. In economics, U.S. trade and investment in Korea composed only a tiny fraction of U.S. foreign enterprise but were a substantial portion of Korea's international commerce. Strategically, the U.S. diplomatic and naval presence helped Korea counter threats from its powerful neighbors, but eventually U.S. leaders, especially Theodore Roosevelt, decided that the balance of power re-

quired Japan's penetration of the peninsula to protect U.S. security interests elsewhere in Asia. Historically, Commodore Robert W. Shufeldt's opening of the Hermit Kingdom in 1882 was a turning point in Korea but only a minor episode for the United States. Geographically, the distance separating the two countries made Korea remote and insignificant to most Americans but made the United States attractive to Koreans, who distrusted bordering nations. Cultural influences and missionary interests reflected and created further disparities. Finally, Chay documents that Roosevelt and other observers of East Asia whom the president respected, such as journalist George Kennan, had a very negative image of Koreans. Chay does not, however, ascribe the asymmetric relationship to individual Americans and Koreans but finds that structural and circumstantial influences prejudiced both sides.

Chay elects wisely not to retell in detail the familiar tale from Shufeldt's amicable negotiations to Roosevelt's hardened realpolitik. Making excellent use of Korean and Japanese sources and an impressive variety of U.S. archives, he adds to the literature a cogent analysis of how this crucial early period of U.S.-Korean relations appeared to Koreans. He does not seriously challenge existing interpretations and accepts the prevailing consensus that power realities and cultural differences left open few alternatives in the course of events. He conveys a sense of regret that Korea failed to strengthen itself as Japan did and China at least attempted.

Chay's focus on Korea's internal weakness is merited, but his approach produces some oversights. The big power rivalry that debilitated Korea's leadership also frustrated U.S. officials. American policy makers scrambled to protect limited U.S. interests against the encroachment of European and Japanese imperialism. Chay acknowledges this dilemma but oversimplifies the U.S. response. He characterizes U.S. diplomats in Asia before Shufeldt as aggressive, for example, yet there were certainly sharp distinctions between an Anson Burlingame and a George Seward. Frederick Low typifies this complexity. Chay describes accurately Low's menacing naval expedition to Korea in 1871 with Admiral John Rodgers's gunboats, but this account fails to note that Low's reluctant resort to force was a singular divergence from his usual forbearance. Such lapses in the analysis do not detract, however, from Chay's genuine contribution to internationalizing the study of U.S.-East Asian relations.

DAVID L. ANDERSON  
*University of Indianapolis*

ANDRE MILLARD. *Edison and the Business of Innovation*. (Johns Hopkins Studies in the History of Technology, number 10.) Baltimore: Johns Hopkins University Press. 1990. Pp. xiv, 387. \$38.50.



Norbert Weiner believed that "Edison's greatest invention was that of the industrial research laboratory." Industrial research had diverse origins, but, when Thomas Alva Edison erected his West Orange laboratory in the late 1880s, there was nothing anywhere else as ambitiously conceived. His aim was to have everything essential to "the business of innovation" right there, from shelves lined with technical journals to the largest "scrap heap" imaginable to the most talented workforce, about one hundred men at the outset. As Andre Millard tells us in this perceptive study, Edison's technique for managing his "boys" was to re-create the culture he had known in the telegraph industry—a shop culture that valued independence, flexibility, and versatility—and Millard excels at depicting "the informality, the conviviality of masters and workers, and the general air of work unbounded by rules or timetables" (p. 31). Many of Edison's inventions had their inception in a sketch passed along to a machinist with instructions to "make this."

The concept of a place like West Orange derived from a paradox: the need to systematize something essentially unpredictable, the creative process. Edison's accomplishments at Menlo Park had shown him that it could be done. But the business of innovation entailed a shrewd sense of the marketplace as well as technical creativity, and, on that score, Edison's record was checkered. In the early days the boys pursued all sorts of "stunts" like flying machines. The sheer technical challenge might override other considerations: "The records of the laboratory show a man happily investing his fortune into scores of experiments carried out more for the fun of doing it than the expectation of profit" (p. 113).

In the emerging milieu of modern corporate capitalism, it could not last. Hence, much of Millard's book details the influx of professional administrators, the decline of the shop culture that had initially defined West Orange, and the shifting emphasis from research to manufacturing and marketing. The shop culture had given experimentation precedence over production engineering. Thomas A. Edison, Incorporated (TAE Inc.), could scarcely afford to do that. By 1914, the needs of a large manufacturing corporation were best suited by "product specific" lab work directed toward something called "engineering services." There was a time clock, just like everywhere else. Millard does not mention Thorstein Veblen, but he is strongly present in this part of his narrative.

After the war, the lab was simply a division of TAE Inc., its chief engineer a division manager. Its equipment was antiquated, the stuff of a museum (which West Orange has in fact become in our own time). It was dwarfed by the Edison factories surrounding it. And there were problems: labor unrest, diminishing markets, and misjudged commercial potentials. The old man himself was part of many of these problems. Millard's book may be read as a tale of retreat from something idyllic. But it is also a suggestive case study

in the changing relationships of technology and business. And, however mournful the fate of Edison's invention factory, one must not forget the significance of West Orange. At the time of Edison's death in 1931, "the landscape of American business was dotted with hundreds of research laboratories" (p. 325).

Millard's ambivalence toward Edison is palpable. But, then, few historians of technology are not ambivalent about Edison, and the Edison Papers project, now unfolding its riches, may not be much help in resolving the profound ambiguities of Edison's career. Millard modestly calls this book "an advance party crossing the vast stretches of the Edison archives" (p. xiii). That it may be, yet his analysis of the rise and decline of the West Orange laboratory is not likely to be superseded.

ROBERT C. POST  
Smithsonian Institution

JOSEPH FRAZIER WALL. *Alfred I. du Pont: The Man and His Family*. New York: Oxford University Press. 1990. Pp. xii, 685. \$27.50.

Joseph Frazier Wall's second major biography of an American industrialist is, unfortunately, not comparable to his first. The reason for this misfortune is more the subject than the biographer. In his outstanding study of Andrew Carnegie, Wall meticulously reviewed and objectively analyzed the career of an ebullient, highly successful entrepreneur whose wealth, fame, and contributions were based on abilities that created the modern American steel industry. Just as steel swiftly replaced cast and wrought iron, so dynamite replaced black powder for high explosives used in mining, railway, and urban construction, and smokeless powder replaced it in firearms and ordnance. And Alfred du Pont "was and always would be a black powder man" (p. 157).

As a black powder man, Alfred played only a minimal role in the creation of the modern Du Pont Company. His most important contribution came in 1902 when he convinced his elders not to sell the company outside of the family. They agreed but "only with the understanding that Coleman and Pierre are associated with you in this proposition" (p. 190). So the three cousins purchased the family's hundred-year-old black powder company for \$12 million.

For Alfred the purchase gave him the chance to revitalize the family black powder mills on the Brandywine. For Coleman and Pierre it provided the opportunity to consolidate and rationalize the nation's production of dynamite and smokeless and black powder in a way that occurred in many American industries at this time. They did so by creating a modern industrial corporation, the E. I. du Pont de Nemours Powder Company, to exploit the economies of scale on a continental basis. Alfred took no part in

the legal and financial arrangements involved in carrying out the consolidation.

Nor did Alfred play any significant role in the essential rationalization and expansion of the company in the years to follow. This is the case despite Wall's claim that "not only did he [Alfred] complete the reorganization and modernization of the original mills at the Brandywine, but also, as vice president and general supervisor in charge of production, he achieved the coordination of facilities throughout the entire rapidly expanding empire" (p. 284). There was no office of production at the Du Pont Powder Company until 1911. Alfred headed the black powder department, which in 1903 accounted for one-third of the company's business, a proportion that declined rapidly. The head of the high explosives department, not Alfred, reorganized and rapidly expanded the dynamite business. The executive in charge of the smokeless powder department did the same for his product.

After leaving the company in 1916 during a bitter legal battle over the sale of Coleman's stock to Pierre, Alfred's business ventures were resounding failures. Only after his third wife's brother came from California to straighten out his finances and assist him with new ones—a major Florida bank and a paper mill—did he prosper.

Alfred's family life was as unrewarding as his business activities. His unhappy first marriage ended in divorce. He then married a younger cousin, the beautiful Alicia Bradford Maddox, as soon as she had divorced her husband. These events and Alfred's vindictive treatment of his first wife and their children alienated Alfred from most of the larger du Pont clan. Soon his second marriage had soured despite the funds and attention he lavished on Alicia. Only after his marriage, immediately after Alicia's death, to Jesse Ball, a school teacher from California, twenty-three years his junior, did he begin to enjoy married life.

Wall records these frustrations accurately but too often regards their creation not as Alfred's doing but as those of others: the du Pont family members who were envious of Alicia, and Coleman and Pierre who seemed intent on driving Alfred out of the company he should have managed. By constantly defending Alfred, Wall misses the opportunities to analyze with care the complex interactions of powerful and strikingly different personalities so as to make understandable the tragedy of a proud man committed to the family into which he was born and to the product it had so long produced. Alfred's was a life thwarted by the creative destruction brought on by new technologies in a dynamic capitalist economy.

ALFRED D. CHANDLER, JR.  
Harvard Business School

THOMAS D. ISERN. *Bull Threshers and Bindlestiffs: Harvesting and Threshing on the North American Plains*.

Lawrence: University Press of Kansas. 1990. Pp. xiii, 248. \$29.95.

Great as definitions, "bull threshers" refers to capital and "bindlestiffs" to labor used in wheat harvesting, and author Thomas D. Isern gives meaning to most other special terms employed in Great Plains harvesting and threshing. An outgrowth of the author's previous book, *Custom Combining on the Great Plains: A History* (1981), this work precedes it chronologically, covering for the most part the late nineteenth century to World War II. Part of the strength of *Bull Threshers*, like *Custom Combining*, is its reliance on personal interviews, business records, and correspondence with men who were part of the great wheat harvest of many years ago. The book has also benefited from the author's extended visits to the small grain-producing provinces of Canada; both sides of the Forty-ninth Parallel have an important and somewhat different story to tell.

Following a chapter on the earlier history of small-grain harvesting, which relies most heavily on Graeme R. Quick and Wesley F. Buchele's *The Grain Harvesters* (1978) and R. Douglas Hurt's *American Farm Tools: From Hand Power to Steam Power* (1982), Isern carefully delineates his subject with individual chapters on harvesting, threshing, human labor, and combines, followed by a summary. Throughout the book he concentrates on the region from the Texas panhandle to Saskatchewan and Alberta. All of the small grains are covered in this important saga in American and Canadian agricultural history, but the emphasis remains wheat and the long harvest beginning in June in the south and ending with snow flying in the Canadian prairies.

Wheat, as a cash crop, was used almost entirely off the farm. The author assumes that his readers understand the requirement of a timely harvest; otherwise crops shattered and were lost. In that sense wheat shared a common problem with the growing of cotton, that other great cash crop. Corn, another great American crop, mostly used on the farm, could stand on the stalk all winter if need be. Farmers were limited in the amount of wheat they could produce by the short time of harvest, whereas corn acreage was limited on each farm by the amount that a farmer could cultivate.

Isern's summary describes the nostalgia that remains for the great wheat harvest before the days of combining, apparent in the many modern threshing bees annually staged throughout the plains and prairies. In considering wheat production in the late twentieth century, the author concludes that changes over the past century show that "the gathering of crops was the crucial climax of a year's work. Knowing this, the plains folk did not hesitate to reform their ways in this area, initially and repeatedly. . . . They took up methods of farming, particularly of harvesting and threshing that worked, and they were

ever willing to discard those ways if they found new ones that seemed to work better" (p. 220).

HOMER E. SOCOLOFSKY  
Kansas State University

MICKEY CREWS. *The Church of God: A Social History*. Knoxville: University of Tennessee Press. 1990. Pp. xvi, 252. \$32.50.

Only one of a myriad of minuscule sects that were swept into the Pentecostal movement in the early twentieth century, the Church of God (Cleveland, Tennessee) has burgeoned into the nation's third largest Pentecostal denomination with global dimensions. This is the first scholarly study of that church, written by one who knows it well from the inside.

In the opening chapter, Mickey Crews sets the emergence of the Church of God in the context of the Populist movement. Convincingly, he argues that the adherents of both movements shared similar beliefs and attitudes. Both, for example, perceived the chaotic, impersonal forces unleashed by urban industrialization to be an unmitigated catastrophe, and both struggled to restore traditional values and traditional social relationships—Populists by political action, Church of God people by personal moral regeneration.

Crews's treatment of the church's origins will likely mislead readers into believing that the Church of God was the fountainhead of the Pentecostal movement and that Holy Spirit baptism with speaking in tongues was normative Church of God doctrine and practice in the late nineteenth century (in fact it became such only after Charles Parham formulated the doctrine about 1901 and after the Los Angeles revival of 1906). Readers may also not realize that the Church of God owes its real foundation and much of its success to its first overseer, A. J. Tomlinson—surely the most charismatic, dynamic, and controversial character in its history—who was forced out of the church under a cloud in 1923. The myth of the origins of the Pentecostal movement in the Church of God and the playing down of Tomlinson's role have long been the staples of Church of God writers. On these points Crews has not escaped his roots.

The transit from an obscure Appalachian sect to a "respectable" major denomination is treated thematically in chapters 2 through 6. The chapter on church government entitled "Christian Democracy" (p. 19) describes a virtual dictatorship under Tomlinson and almost unrelieved authoritarianism thereafter. This is followed by a chapter each on mores (from extreme to moderate asceticism); ecstatic practices (speaking in tongues, faith healing, and snake handling); the status of women (distinctly subordinate); and attitudes toward military service (from pacifism to chauvinism). The concluding chapter recounts the struggle over the past forty years of insurgent progressives to force the church's mostly traditional leadership to

yield grudgingly and incrementally on the approval of higher education, limited ecumenism with right-wing evangelicalism, and racial desegregation within the church.

The most original and important part of this work is its disclosure that snake handling was not, as formerly believed, an unusual practice only in remote Appalachia. Snake handling was nearly universal in the Church of God, and the deaths of snake handlers and of those who refused all medical care were far more frequent than has been recognized. Only in the late 1930s and 1940s did snake handling and rejection of medicine fall into general disuse.

Despite liberalizing trends that appear revolutionary to church traditionalists, the Church of God, relative to American culture as a whole, remains largely exclusivist and culturally backward. That such an institution could have doubled its national membership over the past twenty years to some six hundred thousand today, and that numerous similar churches have grown proportionately, tells us much about contemporary society.

This study will not displace the less objective but fuller history of the Church of God by Charles W. Conn (*Like a Mighty Army* [1977]). Nevertheless, Crews has given us a fine supplement from which all students of Pentecostalism will profit.

ROBERT M. ANDERSON  
Wagner College

CLYDE W. BARROW. *Universities and the Capitalist State: Corporate Liberalism and the Reconstruction of American Higher Education, 1894–1928*. (History of American Thought and Culture.) Madison: University of Wisconsin Press. 1990. Pp. xx, 329. Cloth \$39.50, paper \$17.25.

In *The Higher Learning in America* (1918), Thorstein Veblen described the conduct of universities by businessmen. "Captains of Erudition" had turned knowledge into a merchandised commodity to be standardized, packaged, and marketed. The salable product—a credential—served a series of special interests: enrollment-driven presidents chasing prestige, endowment-driven trustees demanding quietism and conformity; career-driven professionals requiring decorous trophies on résumés. But in Veblen's mind this predatory or entrepreneurial phase of capitalism within universities was soon to be left behind by the evolving industrial or corporate phase. The modern scientist—autonomous, independent, service-oriented, responding to the "facts"—would stand for democratic claims on the public interest. Of course, Veblen's liberal optimism, transcending class relationships and material realities, is unacceptable today. Recent students of corporate liberalism in the Progressive era—Stephen Skowronek, R. Jeffrey Lustig, William Graebner—focus their analysis on the

ideological demands of the military-industrial capitalist state that shut down viable alternatives.

In this new historical-analytic study of higher education in Progressive-era America, Clyde W. Barrow, a political scientist, makes a contribution to the new historiography. Barrow's thesis returns to the critical spirit of Veblen's *Higher Learning*, more so than any work since but without, of course, the readable style. Barrow presents us with a complex piece of work that draws extensively on Marxist theories of modern state building by class elites. His position is that, within the new institution-building arrangements, the academic profession bargained for personal security while quietly giving away academic freedom. Academics only rhetorically maintained a democratic ideal of aspiration, autonomy, and individual achievement. By means of vocational certification, the university system reproduced the existing class structure by sorting people into haves and have-nots, talented and incompetent. Individual achievements did not count in contrast to the usefulness of a disciplinary field to the aggressive capital requirements of the new state.

Barrow develops the argument by moving from the top—where the university was “owned”—downward, but not below policy-making levels. The contents of his research and analysis include social background studies of exclusively well-to-do trustees, the downward statistical trends in academic salaries in the period, administrative responsiveness to vocational and professional introductions to the curriculum as needs for labor changed in the industrial state, the military-industrial-higher education relationship shaped during World War I, the interlocking role of the philanthropic enterprises, the well-publicized academic freedom cases terminating dissenting politics, and the metaphors of efficiency applied by bureaucrats to downgrade professors to proletarian status.

Nearly all of the themes of investigations are familiar. Barrow's contribution is to sustain the argument for the “incorporation” of higher education with an intensity, an interpretive skill, and a depth of research that characterize no previous study from the Marxist perspective. Only the production of knowledge that fit the dominant corporate liberal values, he argues, had a future. The American Association of University Professors organized both late and without great effect. Academic “intellectuals” as a social type did not display the objectivity, scientific skepticism, or critical judgment claimed for professional work.

Barrow's theory-driven work, which assumes the modernization model, is vulnerable to historical particulars that can belie the generalizations. The historian's awareness of unintended consequences, perverse results, subversive relationships, internal defects in the system, and ironic uses of ideological categories have little room for maneuver in this study. Were corporate universities, as Barrow says, less democratic than earlier midwestern populist colleges? Though democratic at the ideological level, those colleges welcomed few Jews, blacks, or women—

groups mentioned hardly at all in this study. Moreover, the intentions of managers and trustees in establishing new programs to classify and warehouse students did not invariably work out according to elite expectations, as David O. Levine has recently concluded. Barrow strains the analogy by declaring that the university in the new state was as much contested terrain as the shop floor. For instance, according to Frank Stricker, despite losing purchasing power on paper, Berkeley professors in the 1920s had a higher standard of living than most of the population. In fact, senior faculty could push for greater differentiation and inequities in faculty pay scales than university presidents. At the University of Chicago, for example, Robert Maynard Hutchins proposed as a matter of principle that junior faculty at the stage of a career with the greatest personal domestic need receive the highest salaries. Senior faculty killed the idea immediately.

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WILLIAM H. WILSON. *The City Beautiful Movement*. (Creating the North American Landscape.) Baltimore: Johns Hopkins University Press. 1989. Pp. x, 365. \$38.50.

William H. Wilson seeks to revive respect for the concept of the City Beautiful in this contentious, uneven, but often-rewarding book. Explicitly revisionist in his purposes, he aims to upset what he calls “traditional planning history” because that history is the story of the winners as told by their heirs commanding the mainstream of the city-planning establishment (p. 3). Previous scholars, Paul Boyer among them, have interpreted the City Beautiful movement as a relatively benign if conservative precursor of the drive for city planning, which evolved from it. Wilson instead defines the City Beautiful movement as an idealistic political cause that became locked in combat with rising exponents of the “city practical”—chiefly engineers, planning specialists, and municipal bureaucrats—who by 1909–10 were eager to discredit the City Beautiful as they surged toward their own long-term influence. In his evident sympathy for the concept of the City Beautiful, Wilson burdens the term itself with misplaced concreteness and thus reads contemporaries' weariness with the term as a concerted attack on the concept. Thus, architect Cass Gilbert, an exemplar of aesthetic conservatism who cherished the neoclassicism of the City Beautiful and championed its other values all of his life, is said to have “flayed” the concept in a 1909 “tirade” in which he actually expressed a few restless doubts about the utility of an overworked phrase (p. 287).

All but three of Wilson's thirteen chapters are largely recycled from his earlier writings, including his *City Beautiful Movement in Kansas City* (1964). The



new chapters are among his best. They include a perceptive evaluation of Frederick Law Olmsted's relations to the City Beautiful and a subtle estimate of the impact on the movement of Chicago's White City of 1893. In both cases, the causal relationship between late nineteenth-century inspiration and the early twentieth-century movement is demonstrated to be more complex than other scholars have assumed. Wilson's critique of the charge that City Beautiful protagonists were practicing elitist social control through their projects is also persuasive.

Most of his chapters are case studies in the politics of the movement as pursued in Kansas City, Seattle, Denver, Dallas, and Harrisburg, Pennsylvania, the only eastern city to merit attention. Oddly, little notice is given to Boston, where Olmsted's last great park system, Edward Bellamy's utopia, and Josiah Quincy's remarkable mayoralty all provided crucial inspirations for the City Beautiful at century's end. More oddly still, Wilson's chapter on Harrisburg passes over in silence the construction of its new state capitol building at the height of its City Beautiful campaign, despite the stress he places elsewhere (as in the case of Denver) on state capitols as anchors for pleasing civic centers, an important City Beautiful concern. The case study chapters do have the merit of clinching Wilson's contention that City Beautiful exponents succeeded in their aims in ratio to their skill at grappling with the demands of democratic urban politics. These chapters also serve to illuminate important aspects of the work of Olmsted's sons John and Frederick as well as that of more obscure landscape planners such as Eugene Schwagerl in Seattle, George Kessler in Kansas City, Denver, and Dallas, and Warren Manning in Harrisburg. The important rhetorical contributions of City Beautiful promoters John Horace McFarland and Charles Robinson win considerable attention.

Wilson notes that one vital strand in the Olmsted legacy—his concern for class reconciliation through urban amenities—was dropped by City Beautiful protagonists. Urban planners at the height of the Progressive era revived this concern, in their attention to playgrounds, housing, sanitation, and transit. It is puzzling that Wilson insists on labeling them the champions of "the city practical" and dismisses their "specious inclusiveness" (p. 257). His spirited defense of the City Beautiful would be more compelling if it did not depend on this sort of argument.

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JULES R. BENJAMIN. *The United States and the Origins of the Cuban Revolution*. Princeton: Princeton University Press. 1990. Pp. xi, 235. \$29.95.

U.S.-Cuban relations deteriorated after the rise of Fidel Castro, with hostility replacing traditional friendship. Leaders in Washington were bemused, as

indicated by President Dwight D. Eisenhower's uncertainty when the press asked, "What is eating Premier Castro?" Since those days thirty years ago, historians have sought to explain what happened. Jules R. Benjamin argues that Washington's reaction was not surprising because North Americans probably never understood the national objectives of the Cuban people. The author believes that, while other historians have attributed worsening U.S.-Cuban relations after 1959 to a revolution betrayed or a policy failure on the part of Washington, the explanation really lies in a long-term examination of these relations. In Benjamin's view the revolution and its aftermath were natural results of a flawed relationship between the two nations, a relationship that broke down because of the antagonism between the U.S. desire for influence and the Cuban desire for independence. Cuban nationalism represented by the new Castro regime effectively resisted Washington's pressure and adopted a policy of socialism, while the United States, assuming that its hegemony and benevolence had benefited both its interests and Cuba's welfare, now found sinister designs or elements that had to be altered or removed. Political, economic, and cultural pressures on Cuba from the United States in the years after 1898 did significant and lasting damage, which went unnoticed by most North American leaders who believed that Americanization of the island was good for all concerned. In the process Cuban moderates on whom Washington relied and who looked to Washington for support lost credibility with the nationalists. As Benjamin notes, "Nationalism came to reside in movements against the state, the military, and the bourgeoisie" (p. 68). Or, as Benjamin asserts later, Washington's attempts to reform Cuban society in the U.S. mold failed to understand Cuba's different culture and as a result weakened the island's conservative institutions, which were capable of acting either as an effective obstacle to U.S. influence or as a block to radical challenges to that influence.

While the U.S. desire to influence Cuban affairs remained constant, there were necessary adjustments to Cuban nationalism. In the period of the Spanish-American War and immediately after, the preference for annexation gave way to establishment of a relationship under the Platt Amendment. With the "Revolution of 1933" came relinquishment of the Platt Amendment and willingness to reform the trade relationship. Nonetheless, Washington's belief that it could still promote, manage, and interfere remained, and in the long run Benjamin believes that Batista-era politicians could not establish their ruling legitimacy any more than could the Machado-era politicians.

As Fulgencio Batista's position weakened, the State Department pressured him to resign and open the way for a moderate and cooperative anti-Batista replacement. These efforts failed in part because of indecision and infighting in Washington and because of the weakness of Batista's opponents who were also

anti-Castro. Benjamin sees this development as part of the dependence of those politicians on the United States. They sought support from Washington, which they believed necessary for success, while Washington was seeking an alternative to Castro that could stand without overt North American intervention. Adjustments to policy made by the United States with Cuba in earlier times were not achieved in the late 1950s, and the new generation of Cuban leaders wanted less U.S. influence over the island. Although Castro's Cuba provided entrance for the cold war into the Americas, the island's difficulties with the United States were of more distant heritage.

Benjamin's views on U.S. dominance, Cuban dependency, Washington's lack of understanding of foreign cultures, and the confusion of cold war issues with national revolutionary aspirations are not new, but his close examination of these issues in Washington's policy toward Cuba provide additional evidence of their role in U.S. foreign affairs. Given the mix in that policy, the clash was inevitable.

WILLIAM KAMMAN  
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MORTON KELLER. *Regulating a New Economy: Public Policy and Economic Change in America, 1900–1933*. Cambridge: Harvard University Press. 1990. Pp. x, 300. \$27.50.

A memorable *Doonesbury* cartoon shows a general explaining to a congressional committee the anticipated difficulties of rebuilding American society following a nuclear holocaust. The prospects are favorable, in his opinion, "providing not too many lawyers survive."

The same disadvantage hobbled the "new economy" that Morton Keller examines in this book: the early twentieth-century American juggernaut of big corporations, big cities, brave new technologies, and progressive regulation. Too many lawyers.

Keller's objective, he tells us early on, is to alter what is at stake in our interpretation of Progressive era and pre-New Deal economic regulation and policy. Setting up Gabriel Kolko's *Triumph of Conservatism* as his principal foil, Keller characterizes the extremes that recent scholarly analysis has contemplated for the 1900–33 period as beneficent but improbable socialism on the left hand and dangerous nationalist corporatism on the right.

Rather than dispute the Kolko school's contention that malign corporatism eventually prevailed, Keller proposes entirely new terms of controversy. The economic, legal, regulatory, and "policy" history of the first third of the century, he argues, should be understood as a complex combination of persistence of "preexisting values, interests, [and] procedural and structural arrangements" and pluralism: "an expanding, roiling aggregate of interests, issues, institutions, ideas" (p. 3).

As a dichotomy specific to early twentieth-century America, persistence and pluralism put me off. What era can avoid being made up of some combination of the two? Fortunately, Keller uses them more as a method of arguing than as an explanatory framework. His real interest, as the title announces, is regulation, and it is his expansion of the realm of regulation to encompass bureaucracies, legislatures, lawyers, and courts at all levels of government that is the book's very important contribution.

Keller approaches regulation head-on. In chapters entitled "Regulating Trusts," "Regulating Utilities," "Regulating the Countryside," "Regulating the City," and others, he builds up a wonderfully practical and intelligent analysis. In each area, familiar Progressive era legislation and regulatory mechanisms form the beginning of the story. Keller then moves to the succeeding experiences, in which forward-looking principles and powers of enforcement are blunted, twisted, and diverted by attorneys brandishing such traditional weapons as the injunction and the law of conspiracy and by state legislatures and state courts motivated most importantly to aid and protect the leading interests of their local economies.

The consequence—as Keller concludes at the end of almost every chapter—is not the marble cake of interpenetrating persistent and pluralist strata advertised in the introduction but rather ambiguity: a chaotic jumble of precedents and pronouncements almost entirely devoid of a unifying philosophy or a consistent train of reasoning.

Why has it taken until 1990 to produce a properly detailed and comprehensive analysis of regulation of the early twentieth-century economy (and well-written to boot)? Because only by immersing oneself in the morass of state and local legislation, court cases, and judicial decisions, as Keller has done, is it possible to flesh out the regulatory melodrama in all its grotesquery. For this, we are greatly in his debt.

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JOYCE SHAW PETERSON. *American Automobile Workers, 1900–1933*. (SUNY Series in American Labor History.) Albany: State University of New York Press. 1987. Pp. xiv, 231. Cloth \$54.50, paper \$17.95.

In studies of automobile workers, the 1930s and 1940s are favored over the first three decades of the century. The earlier period offers neither the drama of the sit-down strikes nor the inspiration of victorious unionism. The period's surviving records, moreover, have seemed better suited for business and management research than for the new labor history. Joyce Shaw Peterson's book is thus a welcome addition to the literature.

Peterson has written, above all, a handy reference work, assembling useful information on the staple topics. Successive chapters summarize the industry's

growth, the composition of the work force, changes in manufacturing practices, wages and working conditions, living standards, labor unrest, early unionism, and the impact of the Great Depression on workers' lives and labor militancy. Within each topic, Peterson surveys evidence but avoids controversy. Clashing interpretations of automobile mass production by Michael Piore and Charles Sabel, Wayne Lewchuk, and David Gartman, for example, do not appear in the account of changes in auto work. The review of wages and working conditions, similarly, does not engage such rival accounts of corporate labor policies in the 1920s as those offered by Richard Edwards and William Lazonick. Only when evaluating the roles played by radicals and by relatively skilled, native-born workers in the strikes and union drives of the early 1930s does the author hint at debates among other scholars.

Within these self-imposed limits, Peterson presents some valuable material. She uses municipal surveys, for example, to document successive waves of migration into Detroit. Thus, we find data showing that before 1910 the city's newcomers were largely native-born whites; between 1910 and 1920 they were mainly Southern and Eastern European immigrants; and during the 1920s the majority were southern blacks. Peterson also uses city surveys to show in detail that auto workers' wages and living standards were superior to those in most other industries and that by the mid-1920s auto companies afforded their employees a degree of security that was unprecedented for less skilled labor. Extensive use of company archives, finally, allows Peterson to give new examples of auto manufacturers' employment practices, output restriction among auto workers, and other familiar features of labor relations in the nonunion era.

It is in compiling information of this type that the author does her greatest service. In addition, Peterson presents an argument about the weakness of auto unionism prior to 1933. The argument, although not new, does put her surveys of the evidence on specific points to good use. High wages and paternalistic welfare schemes made alienating factory labor more bearable, and corporate anti-unionism and ethnic and racial divisions among workers made collective resistance difficult. By the early 1930s, however, ethnic cleavages had receded, and the depression had eliminated high wages and welfare programs. With these bases of worker accommodation undermined, Peterson concludes, the stage was set for labor militancy and unionization after 1933.

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JOHN MAXWELL HAMILTON. *Edgar Snow: A Biography*. Bloomington: Indiana University Press. 1988. Pp. xii, 343. \$25.00.

John Maxwell Hamilton's seminal biography of Edgar Snow is an important contribution to the study of American journalists in China.

Born in Missouri in 1905, Snow graduated from the Missouri School of Journalism and traveled to China, as a stowaway, to report on revolution and change, which he did with idealism and personal fervor. He was part of that group of journalists, including Theodore White, Annalee Jacoby, and Graham Peck, who were committed to a cause. He traveled to Mao Zedong's guerrilla redoubt in Yen'an in 1936 and stood with Mao on the balcony of Tienanmen in 1970. Snow died on February 15, 1972, just four days before Richard Nixon left on his historic trip to China. His ashes are divided between the grounds of Beijing University and the shore of the Hudson River.

For most Americans, Snow is famous for his book *Red Star over China*. Snow's sympathetic and somewhat romantic introduction of the goals, history, and personalities of the Chinese Communist revolutionary army established Mao Zedong as a Chinese hero in the American press. For his efforts, Snow was at first received as a popular and insightful author. Later he was so vilified for helping to "lose China" that he felt like an Ishmael in his own country (p. 211). He spent the last years of his life in self-imposed exile in Switzerland.

Hamilton, in chapter 3, narrates for the first time the compelling story of how *Red Star over China* was written and how it was received in the United States. This excellent chapter should be read by journalists as well as by historians.

Hamilton depicts Snow as a midwestern idealist and humanist who was sympathetic to the plight of the common man. Despite his personal commitment to the Chinese people, he was still a professional journalist who did not let his romanticism totally dictate his reporting. His objectivity and realism made him an enemy to ideologues. He was accused of being a spy in China and a fellow traveler in the United States. Because of the anti-Communist atmosphere and Snow's own apologetic attitude for the Chinese regime, his second major effort on China, *The Other Side of the River, Red China Today* (1962), was a public and financial disaster.

The sources for the biography are profuse and rich. Hamilton had access to Snow's papers, and he interviewed family members, journalists, historians, colleagues, and friends. He also used documents obtained through the Freedom of Information Act. He enhanced his work with two trips to China. His narrative, though thick with detail, is not dull or heavy.

Hamilton is fascinated with Edgar Snow's journalistic career and idealistic global concerns. Like Snow, Hamilton was a journalist in the United States and abroad. He has had broad international experience working with the U.S. Agency for International Development and with staffs of the House Foreign

Affairs Committee and World Bank. His doctorate is in American civilization from George Washington University.

In some ways he shares Snow's limitations: he has a lay person's knowledge of Chinese history and minimal Chinese-language skills. He did not conduct interviews in great depth. Snow complained that his editors did not provide enough maps. Hamilton, too, has inadequate map coverage. This study could be supplemented with a work more familiar with China and the problems of reporting in China: Stephen R. MacKinnon and Oris Friesen's *China Reporting: An Oral History of American Journalism in the 1930s and 1940s* (1987).

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RONALD D. COHEN. *Children of the Mill: Schooling and Society in Gary, Indiana, 1906–1960*. (Midwestern History and Culture.) Bloomington: Indiana University Press. 1990. Pp. xiv, 280. \$35.00.

Gary, Indiana, had perhaps the nation's most famous public school system earlier in this century. Gary's schools were among the half-dozen highlighted in John Dewey's *Schools of To-Morrow* (1915). In 1929, the *Chicago Tribune* described Gary's public schools as containing "20,000 of the happiest, healthiest, busiest, most interested and most interesting children in America" (p. 106).

Created by Superintendent William Wirt (1874–1938), Gary's "platoon" system rotated students through activities in supermodern facilities during an extra long school day and year, giving schools unprecedented opportunities to regulate children's lives. The "work-study-play" plan appealed to the three major streams of progressive educational theory: that schooling attend to the needs and interests of the "whole child," hence the manual training and industrial arts programs, school gardens, swimming pools, auditorium and gymnasium events, religious instruction by the city's ministers, and a much enriched academic curriculum; that the school function as the engine of democratic social progress, entailing comprehensive after-school, weekend, and summer programs for adults and expanding social and psychological services for children; and that schooling pursue the efficiency, economy, and rationalization so optimistically endorsed during that era. From 1907, Gary "began to develop an educational philosophy that would link past to present, children's needs with social and economic imperatives, and small town values with urban realities" (p. 20).

The contradictions inherent in this eclecticism and in the coalition politics of progressivism surfaced as these missions were implemented. Moreover, no school plan, however accommodating, could withstand the successive assaults of the Great Depression, cold war, and the often ugly political economy asso-

ciated with demographic changes. Nationwide, progressive education unraveled as a social movement. Gary's experiment was first compromised and then dismantled piecemeal. Ronald D. Cohen's case study of Gary and its schools uncovers still more contradictions and now familiar ironies. For one, the political Left generally supported Gary's experiment, despite its connections with the United States Steel Company and evidence that pro-labor, liberal, and radical opinion elsewhere opposed "industrial education" as limiting lower-status children's opportunities. Cohen explains that "reformers continually saw different things in the enigmatic Gary Plan" (p. 51). Yet Cohen remains uncertain that the intentions or effects of Gary's approach were as evil as revisionist historians have deterministically assumed: "While my inclination is to veer leftward, perhaps interpreting the schools as conscious or unconscious tools of the corporate hierarchy, I have rather an ambivalent view of what happened in Gary" (p. xi). While disavowing a pluralist historiography, Cohen concludes that, within the limitations of corporate capitalism, "there has been a general give and take among various and competing individuals, groups, and organizations. And, perhaps, children have, occasionally, reaped the benefits" (p. xii).

Another irony was the specter of European immigrants, sufficiently "Americanized" to employ the assertive, "democratic" strategies of longer-settled citizens and supporting their children's strike against greater integration in Gary's sole racially mixed school; strikers protested using students as "'guinea pigs' in race relation experiments" (p. 179). The black press retaliated with references to the "scum from Europe," who were cashing in on their fair skins to "gain opportunities and rights that are basely denied purely American citizens of color" (p. 179).

Cohen has written before on Gary's place in the intellectual and social contexts of progressive education. His chief contribution here is extending Gary's story nearly to the present. This adds significantly to our slim knowledge on how school boards and administrators dealt with the depression, the disruptions of World War II, the red baiting during the cold war, and the "white flight" as many urban school systems became predominantly black. With the end of its unique platoon system, Gary came to stand for "rust belt" city school systems.

Although Cohen's title highlights Gary's children, there is little evocation of what it felt like to work, study, or play in the city's schools, especially surprising given the several commissioned external studies of Gary's experience. Were there no diaries or letters of teachers, students, or parents to exploit along with newspaper files, official reports, and Wirt's voluminous papers? It is a small wonder that Cohen's book ends so uncertainly: "Perhaps a return to the old work-study-play plan would make the elementary schools at least more interesting, if not actually in-



creasing the students' academic, cultural, and social skills. Perhaps" (p. 224).

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DORIS GROSHEN DANIELS. *Always a Sister: The Feminism of Lillian D. Wald*. New York: Feminist Press. 1989. Pp. x, 207. \$24.95.

Progressive reformer, suffragist, activist in the peace movement, founder of Henry Street Settlement, Lillian Wald was a significant turn-of-the-century Progressive reformer who has not until recently received the historical attention she deserves. Doris Groshen Daniels takes important steps to establish and affirm the significance of Wald's talents and achievements. Thematically rather than chronologically, she documents and explains Wald's public efforts: her campaign for the professionalization of nursing, for child labor and minimum wage legislation, for trade unionism, for an intensified investigation of the brutal poverty that accompanied American industrialization. Like Jane Addams and other social settlement activists, Wald lobbied, canvassed, and cajoled major public leaders. With her Henry Street Settlement colleagues, she published countless articles, gave fund-raising speeches, joined picket lines, initiated conferences, and thus not only responded effectively to the needs of the New York immigrant community but also provided an important training ground for contemporary politicians and reformers and in the process served as a crucial link between feminists and immigrants, and between immigrants and the American middle class.

Daniels does not approach her subject with sentimentalized or heroine-worshiping respect, however. She notes some limitations—Wald's occasional inconsistencies on the race issue, for example, and her expedient though sometimes compromising tendency to please. And she describes Wald's forceful public presence—as a superbly skilled administrator, a tough-minded fund raiser, a pragmatic idealist who was neither naively altruistic nor simplistically responsive to urban-industrial concerns. And yet Daniels's presentation leaves me with some reservations. I find myself wishing for more lifelike character portrayals, more richly documented stories that would explain the depth and passion of Wald's struggle. Daniels notes Wald's "dynamic personality," for example, her "healing power," her "inordinate capacity for loving people," her close, respectful relationships within the immigrant community (pp. 51–52). She does so only briefly and somewhat vaguely and abstractly. Similarly, she acknowledges Wald's supportive female friendships, with Florence Kelley, for example, and with Jane Addams and Lavinia Dock. But instead of allowing readers to taste the richness of their correspondence, instead of documenting or

demonstrating the importance of their networks of sisterly affection, Daniels rather defensively suggests that their "flowery" language, their "effusively loving and sentimental" private correspondence should not distract us from the importance of their public work (p. 73).

The strength of Daniels's work thus emerges from her treatment of Wald's public efforts. She establishes their importance in historical context. She demonstrates the ways in which her feminist convictions were integrally related to her work with contemporary reform-oriented associations—with the NAACP, the Women's Trade Union League, the National Community League, the American Association for Labor Legislation (to name but a few). But she is less persuasive in her efforts to portray the full depth and power of Wald's feminist or woman-centered life.

MARY A. HILL  
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ARTHUR ZIPSER and PEARL ZIPSER. *Fire and Grace: The Life of Rose Pastor Stokes*. Athens: University of Georgia Press. 1989. Pp. xv, 348. \$35.00.

In the early years of this century, a dozen or so women came to public notice as advocates for one or another of the radical ideas and movements that characterized that time. Rose Pastor Stokes was among them. A Socialist, later a Communist, her fame was initially based on personal circumstances, for she was a poor Jewish immigrant girl who, through talent and luck, went from a Cleveland cigar factory into the upper classes: in 1905 she married the very rich, socially prominent, and liberal-minded James Graham Phelps Stokes. The newspapers, avid for the Cinderella story, also followed the subsequent life of the couple as they embarked on a political odyssey that took them from the idealism of settlement house work into the Socialist party where they engaged in every radical cause and with every radical personality of the day.

Arthur Zipser and Pearl Zipser assert that during the two decades of the Stokes marriage, Rose was "more famous" than such female contemporaries as Emma Goldman, Elizabeth Gurley Flynn, or Margaret Sanger; they note that whereas these other women have had their biographies written (in fact, Flynn has had no full-scale biography), Stokes has so far been only a historical footnote.

The Zipsers have written about their subject from the point of view of colleagues or comrades once removed. They have produced a hagiography. Not only do they skim over the content of Rose's causes without illumination—for an understanding of the radicalism of the first decades of the century one may turn to numerous other sources—but, worse, the authors view their subject entirely by her own lights. There is no doubt that the Zipsers admire Stokes deeply, as do their witnesses who testify to her qual-

ities of ethereal beauty, artistic temperament, intelligence, idealism, and sincerity of purpose. They are also unaware that by the evidence of her own words, Stokes seems naive, dogmatic, humorless, and self-regarding. For instance, she wrote to Upton Sinclair in 1911, lamenting that she had no time to devote herself to writing because "the platform work would suffer much. Your 'Jungle' may reach the readers, but I have the joy of reaching the earnest men and women who have never so much as *heard* of your book—who may yet read much someday because I have reached them with *my* message" (p. 145).

The Stokes marriage slowly disintegrated as Graham moved to the political right and Rose into the Communist party. As a Communist, her faith got the better of reason as when she defended the Bolshevik repression of the anarchists; if she were a dissident in the Soviet Union, she wrote, "I myself would want to be put in jail . . . tucked away in some nice double-locked cell until the danger from my muddled, obstinate self is over" (p. 227). She attacked her old hero Eugene Debs when he protested the Bolshevik repressions. The Zipsers comment only that "the old man deserved better" and that despite disagreements, Debs remained close to the Communists.

After a bitter divorce in 1925, Rose soon married V. J. Jerome, a man almost twenty years her junior; in the early 1930s he would emerge as the party's cultural commissar in charge of keeping the intellectuals in line.

Rose's last years were spent in penury and illness. After a long battle with breast cancer, she died in 1933 at the age of fifty-four. Her unfinished autobiography does not seem to have been a major source for the authors. One of the few quotes they offer from it may provide a clue as to why it was not more helpful: "I had thrown myself into strikes with the fury of a true soldier in the ranks of my class. Nothing was too difficult; all-night picketing, all-day activity, at times no sleep, no food; it did not matter. Often I kept going until I broke and had to be dragged away from the scene of battle. There came no call of any kind to aid in the struggle to which I did not respond with joy. Another blow at the enemy, another and another!" (p. 162).

DOROTHY GALLAGHER  
New York, New York

ELLEN FITZPATRICK. *Endless Crusade: Women Social Scientists and Progressive Reform*. New York: Oxford University Press. 1990. Pp. xv, 271. \$35.00.

Students of Progressive-era reform may be familiar with the names of Edith Abbott, Sophonisba Breckinridge, Frances Kellor, and Katharine Bement Davis but know little detail about their careers. In their own time, the four, all social scientists trained at the University of Chicago at the turn of the century, were

well known for their work on the social and economic conditions of the working classes, especially of women workers. Ellen Fitzpatrick's study explores their intellectual development and professional experiences. Her book not only compensates for the absence of much previous attention to these women but shows how the four, thinking about women and acting for women, helped shape public policy.

In the 1890s, University of Chicago social scientists were applying scientific methods to the nation's growing problems among the immigrant and working classes. The four women found mentors and inspiration in this setting, which Fitzpatrick describes through a collective portrait of such lights as Dean of Women Marion Talbot, economists J. Laurence Laughlin and Thorstein Veblen, sociologist Albion Small, and political scientist Ernst Freund. The women's career paths were not easy. The male-dominated university seldom awarded women prestigious fellowships or advanced them as faculty at major research institutions. Fitzpatrick skillfully interweaves the four women's experiences as they struggled against nineteenth-century prescriptive norms for women's educations and careers.

Prevailing over many obstacles, the women led productive lives. Davis became superintendent of a new women's reformatory in Bedford Hills, New York, then New York City's first woman commissioner of corrections, and later chair of the parole board. She ended her career as head of John D. Rockefeller, Jr.'s Bureau of Social Hygiene. Kellor translated her sociological studies on women's unemployment and the problems of immigrant workers into leading positions with voluntary associations, the Progressive party, and New York State. Later she founded and was vice president of the American Arbitration Association. After completing dissertations in economic history, Abbott and Breckinridge taught in Graham Taylor's School for Civics and Philanthropy, which eventually merged with the University of Chicago. In addition to teaching many of the nation's future social workers, the two women continued to do research and write on women's and children's work conditions. Throughout their careers, all four women were politically active. Using their research as the basis for developing social policy, they worked through voluntary associations or political parties for much of the advanced social legislation of their day.

Fitzpatrick casts her portraits of the four reformers in a critical framework. She admires their insistence on empirical research as a basis for reform and their sensitivity to gender. But she is also aware of the limits of their approach, especially of its potential for undemocratic, even oppressive, treatment by social engineers of less privileged subjects. Her thoughtful consideration of these issues enhances her deeply researched and well-written study. This book signifi-

cantly advances our understanding of the impact of women intellectuals on Progressive-era reform.

ELISABETH ISRAELS PERRY  
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ELLEN CONDLIFFE LAGEMANN. *The Politics of Knowledge: The Carnegie Corporation, Philanthropy, and Public Policy*. Middletown, Conn.: Wesleyan University Press. 1989. Pp. xv, 347. \$30.00.

The Carnegie Corporation of New York, founded in 1911, was the last and largest of the philanthropic institutions established by Andrew Carnegie. Ellen Condliffe Lagemann explores its history from 1911 to 1982, although, as she states, her study is more selective than comprehensive. For example, the extensive grants made by the foundation outside the United States are excluded from consideration. She concentrates on a relatively few domestic grants that she analyzes as to their degree of effectiveness in the public policy area—hence, the title of the book. In the introduction the author states that her work involves or turns on three questions: which fields of knowledge and which approaches therein are deemed important and authoritative by an elite, how this knowledge is transmitted in a democracy, and how a creating and transmitting elite develops. Placing the Carnegie Corporation within this focus, Lagemann divides her history into four periods.

The first period (1911–19) covers the years when the foundation's program of giving was dominated by Andrew Carnegie. His penchant for funding libraries and church organs, ranging into millions of dollars, is dismissed in seventeen pages as having little influence on public policy.

Following Carnegie's death in 1919, the second period (1919–23) is approvingly termed one of "scientific philanthropy." Led by Elihu Root, James R. Angell, and Henry S. Pritchett, the foundation is described as moving from a passive role to a pivotal one in the creation of such organizations as the National Research Council, National Bureau of Economic Research, and American Law Institute, which in turn influenced public policy in science, economics, and law.

The third period (1923–41) under the presidency of Frederick P. Keppel is labeled "cultural philanthropy" and is "in some ways like the first period" (p. 7). With funding for the arts, adult education, and libraries at the core of Keppel's domestic program, the foundation's conception and method of giving is pejoratively dubbed elitist, male oriented, and of limited influence in the forming of public policy in these fields. The one study funded by the corporation that had a profound and, it is noted, unexpected influence on public policy was the one that resulted in the publication of Gunnar Myrdal's *An American Dilemma*.

The fourth period (from World War II to 1982) is

termed "strategic philanthropy" and covers the presidencies of Devereux C. Josephs, Charles Dollard, John W. Gardner, and Alan Pifer. The foundation's program and projects of the period, particularly those developed and carried out under Gardner and Pifer, are admirably viewed as highly influential in the public policy area. Emphasized during their administrations is the foundation's role in the formation of national educational policy, effectively demonstrated in the origination and work of the Carnegie Commission on Higher Education and the Carnegie Commission on Educational Television.

Lagemann's work provides a fresh and enlightening approach in its examination of the efficacy of the foundation's aims, priorities, and programs from the standpoint of their success or failure in influencing public policy. It should be remembered, however, that there are, and in the past have been, distinct differences as to what that public policy should be. It could be argued, for example, that Andrew Carnegie was not "naïve and romantic" (p. 23) but indeed was trying to beneficently influence public policy in his insistence that citizens stock and maintain the library buildings that he and the foundation provided. Despite an occasional such judgment on her part, the author provides an interesting and interpretive account of some of the most notable domestic programs of one of the oldest and most important U.S. foundations. This significant work makes a distinct contribution to the literature on the story of our great foundations, a relatively neglected segment of the history of philanthropy.

JOSEPH C. KIGER  
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STEVEN C. WHEATLEY. *The Politics of Philanthropy: Abraham Flexner and Medical Education*. (History of American Thought and Culture.) Madison: University of Wisconsin Press. 1988. Pp. xix, 249. Cloth \$37.50, paper \$14.95.

Beginning in 1911, Abraham Flexner, educational reformer, transformed American medical education. As Steven C. Wheatley recounts it, Flexner took medical education out of the hands of local medical elites and grounded it in the new discipline of clinical science. Although medical schools had incorporated scientific training and research, the clinical side was dominated by part-time practitioners with lucrative private practices. Flexner's report, *Medical Education in the United States and Canada* (1910), prompted Frederick T. Gates, the philanthropic adviser to John D. Rockefeller, to ask his advice about how best to reorganize American medical education. Thus began Flexner's career on the General Education Board (GEB), a Rockefeller philanthropy. From 1912 to 1928, as assistant secretary, secretary, and ultimately head of the Division of Studies and Medical Education, Flexner single-mindedly pursued the goal of

inducing medical schools through large endowment grants to reconstruct their clinical faculties with full-time professors.

In the process, he directed \$78 million to Johns Hopkins, Washington University, Yale, Rochester, Vanderbilt, the University of Chicago, and Columbia. He also enraged those who refused to alter their ways. Ultimately, his enemies, especially Dean David Edsall at Harvard, united with the rising generation of philanthropic managers in the Rockefeller Foundation to force Flexner out. He retired, only to take as his next project the Institute for Advanced Study at Princeton.

Wheatley places Flexner's ideology and actions within the complex history of foundation development. Flexner's career came at the transition point between the era of personal advisers and that of impersonal managers, prior to the period in which the federal government began its major investment in medical research and education. Wheatley links Flexner's efforts to the history of medical education and specific medical schools. He writes with care and discrimination about these complicated subjects, demonstrating a mastery of their immense literatures. Finally Wheatley sets these complex interlocking narratives within the frame of the "politics . . . of the management of organized knowledge" (p. ix), a campaign by the philanthropic managers of the young foundations to nationalize and bureaucratize institutions that had heretofore been local and personal. His case study extends the work of Barry D. Karl and Stanley N. Katz in a fruitful direction.

Wheatley did not set out to write a biography, but, in a history of the politics of philanthropy, he must pay attention to the intersection of personal life, social context, and political forces. The author minimizes college ties, family, gender, and ethnicity and thus misses key elements in Flexner's story. Did the fact that Flexner was an alumnus of Johns Hopkins lead its medical school to be favored at the expense of others? What did Flexner's relation to his brother Simon, head of the Rockefeller Institute, mean to him and to others? Was the relation of Flexner's effort to transform medical education to decisions to limit or exclude women's enrollment in some schools merely coincidental? Was anti-Semitism the reason Flexner was passed over for the head of the GEB? Wheatley passes over without comment the many references of Flexner's critics to the Old Testament. He quotes F. G. Shattuck's remark—"There are circumsized [sic] folk in NY, circumsized alike in pecker and intellect" (p. 82)—without probing its anti-Semitism. Wheatley's prose tends to abstraction, making the narratives difficult to follow and putting Flexner's story at great remove. The man himself wrote pungent sentences; in describing his actions, Wheatley can be bland.

Despite abstract prose and unexplored dimensions, the book has considerable merit. It offers a full exposition of Flexner's medical education reform and the ideology that underlay it, a history of the Rock-

efeller Foundation up to the 1950s, and informed summaries of changing medical school organization and practice.

HELEN LEFKOWITZ HOROWITZ  
Smith College

CHARLES J. MALAND. *Chaplin and American Culture: The Evolution of a Star Image*. Princeton: Princeton University Press. 1989. Pp. xxi, 442. \$29.95.

Adopting the premise that the work of an artist can provide useful insights into a nation's cultural history, Charles J. Maland examines the historical interplay between one of our most prominent artists, filmmaker Charles Spencer Chaplin, his film persona, "Charlie," and American society. This book is both a biography and a historiography of one of Hollywood's best-known and most popular stars.

Going beyond David Robinson's excellent biography, *Chaplin, His Life and Art* (1985), Maland traces the creation of the star image and the reaction of American society to Chaplin and "Charlie" from the time of his arrival in the United States in 1912 until IBM's successful personal computer advertising campaign based on the little tramp character. For much of his career, Chaplin was able to control his image through what Maland terms "media texts": his films, studio promotional material, publicity (what the press knew about the star), and criticism; however, publicity about his marriages, paternity suit, and political interests eventually alienated the American public. Subjected to an investigation by the Federal Bureau of Investigation (FBI) and vigorous smear campaigns, which impugned his loyalty, by gossip columnists such as Hedda Hopper, Chaplin finally opted to move from the United States where he had worked for more than forty years. Twenty years later, the industry welcomed him back with an Oscar and a place on the Hollywood Boulevard Walk of Fame.

In his analysis, Maland clearly demonstrates the importance of the historical context within which Chaplin worked and his films appeared. The historical events of World War I, the transition to talking motion pictures, the Great Depression, World War II, and the McCarthy and Cold War era all affected the public's perception and reception of Chaplin and help explain the shifts in his career and popularity.

In addition to a thorough review of the films, publicity, Chaplin's own writings, and the secondary literature, Maland obtained the FBI files on Chaplin. Maland confirms the conclusion reached by Robinson in the earlier biography that the files reveal more about the methods of the FBI than about Chaplin. In particular, Maland traces a clear pattern of leaks and exchange of information between the bureau and Hollywood gossip columnists who viewed Chaplin as a supporter of left-wing causes. Maland also makes it equally clear, however, that it was not until Chaplin



became less vocal in support of progressive causes that his star image began to rise once again and culminated in his Oscar award in 1972.

Maland's combined biography and historiography is clearly a standard to challenge future film scholars.

JOHN S. SCHUCHMAN  
Gallaudet University

GARY GERSTLE. *Working-Class Americanism: The Politics of Labor in a Textile City, 1914–1960*. (Interdisciplinary Perspectives on Modern History.) New York: Cambridge University Press. 1989. Pp. xii, 356. \$39.50.

In this study of the rise and fall of labor influence in Woonsocket, Rhode Island, Gary Gerstle offers a compelling exploration of working-class radicalism from World War I through the New Deal era. Combining a community study framework with a close analysis of political language, Gerstle's book operates on two levels—at once a well-grounded industrial and political narrative and an extended argument about the multiple uses of "Americanist" rhetoric and ideals. If the two themes do not always connect, the work nevertheless turns a painstaking monograph into a convincing handle on major problems in twentieth-century social and cultural analysis.

Union building in this New England woollens center occurred amid a fascinating clash of cultures. A split between self-made entrepreneurs and absentee French textile magnates investing abroad to elude the McKinley tariff mirrored a sharper division in the work force between the numerically dominant and Catholic-traditionalist French Canadians and a secular, socialist Franco-Belgian enclave among the town's skilled operatives. In the pre-World War I years, the French connection between millowners and a conservative clergy served the cause of *la survivance*, or a politically quiescent cultural survivalism. By the mid-1920s, however, the persistent efforts of the Belgian radicals (passing organizationally from the Industrial Workers of the World to the American Federation of Labor) combined with a dissolution of unitary authority in the French-Canadian community opened the possibility for a broader working-class politics.

For at least a decade after its founding in 1931, Woonsocket's Independent Textile Union (ITU), a breakaway from the inept United Textile Workers, offered a most expansive (albeit sexist) version of radical Americanism operating on and off the shop floor. Piloted by the tough, Belgian socialist Joseph Schmetz and the Jewish-Communist recruit Lawrence Spitz—while drawing key support from second-generation French-Canadian mulespinners—the ITU parlayed the National Recovery Administration's labor code and unqualified victory in the 1934 textile strike into a formidable industrial union with far-reaching social impact. By 1941, a union shop, workers' control of many shop-floor decisions, higher than average wages, extended benefits, and housing

and education programs (including a union library!) defined a distinctive "union" way of life in Woonsocket. The ITU's very success, however, ultimately plunged it into internal conflict. A French-Canadian leadership cadre, with support from pro-union clergy, wielded an anticommunist stick to rid the ITU of its proud Old Guard by the end of World War II. Forsaking the defensiveness of *la survivance* for their own brand of Americanism (but one rooted in ethnic communalism, not social-democratic universalism), the new leaders might have inaugurated a new era of ITU dominance in Woonsocket. Precipitous decline of the key local industry, however, left the union as well as the town in anguish by the early 1950s. Overall, Gerstle brilliantly dissects this extended cycle of union building and ultimate institutional decline. Harnessing retrospective oral testimony to a probing review of contemporary records, he is especially sensitive to political shifts in the union's leadership corridors.

The author's penchant for fine political-cultural distinctions within the Woonsocket working class, however, does occasionally exceed his evidentiary base. This is particularly a problem in connecting the embittered leadership fights of the late 1930s and early 1940s to the social history of the community. The complex organizational issues (for example, whether to merge with the Congress of Industrial Organizations), local party politics, and personal rivalries do not always align themselves neatly along Gerstle's preferred ideological axes of "progressive Americanists" vs. "ethnic corporatists." No doubt because of the unevenness of his oral informants, Gerstle's grip on the French-Canadian perspective on union events also seems less sure than that for the Old Guard. But these are small reservations about an important and challenging book.

LEON FINK  
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HERBERT F. MARGULIES. *The Mild Reservationists and the League of Nations Controversy in the Senate*. Columbia: University of Missouri Press. 1989. Pp. xiv, 300. \$39.00.

Herbert F. Margulies, who some years ago provided us with an admirable biography of Senator Irvine Lenroot of Wisconsin, one of the more prominent mild reservationists in the debate over the League of Nations, has now turned his attention to the mild reservationists as a group. Although historians of the ratification fight over the Versailles Treaty have charted the actions of Woodrow Wilson, Henry Cabot Lodge, and the "irreconcilables" in considerable detail, this is the first systematic study of the group that included such important figures as Lenroot, Frank Kellogg, Porter McCumber, and Charles McNary. As

such it is a valuable addition to the literature on the treaty fight.

Margulies is clearly sympathetic to the mild reservationists. He portrays them as perspicacious men who understood the constitutional complexities of international organization and pursued the one course likely to result in U.S. participation in the League of Nations. He defends them against the charge that they counted for little and were so timid that they must share in the blame for the League's rejection. Instead, Margulies depicts a group of men intent on effecting ratification who worked more effectively toward that end than did either Wilson or Gilbert Hitchcock, the Democratic leader in the Senate. Believing that Wilson's public insistence on unreserved ratification was unrealistic and arguing from the premise (correct, I am convinced) that Lodge had the votes to reject the treaty if satisfactory reservations were not appended. Margulies contends that the mild reservationists in finally uniting behind reservations "took the position most likely to bring Senate approval, in a form as close to Wilson's own as was politically practical" (p. 7).

Just as he demonstrates that his protagonists were sensible men who knew what they were doing, the author himself shows particularly good sense in his discussion of the reservation to Article X. Understanding that the reservation to Article X struck at two key ingredients of collective security—predictability and universality—Margulies argues that such a reservation was a necessary condition to ratification. Most senators, including the majority of the mild reservationists and all of the moderates, were unhappy with the commitment to coercion inherent in Article X and simply would not accept it without substantial qualifications.

Though soundly argued and based on prodigious research, this book does present a few difficulties, one inherent in the subject matter and the others in the author's approach to his subject. When dealing with a body of men as notoriously independent as U.S. Senators, division of them into readily defined groups is precarious business. Margulies believes that the traditional distinction between mild and strong reservationists is an oversimplification, and he is probably right about that. To discover, however, that there were eight or so senators in between who were all at one time or another considered mild and to call them sometimes moderates and sometimes middle grounders is somewhat confusing and reminds us to be wary about viewing senators who happen to act together as participants in any cohesive bloc.

Readers should also be aware that there are some dangers inherent in the author's sympathetic approach to his subject. It is clear that he not only holds the mild reservationists in high regard but also believes that the United States should have joined the League. This sometimes leads him into a preoccupation with blame and into problematic judgments, for example, when he puts the mild reservationists on the

Left and writes that they were "forced to make the best deal possible with those to the right of them in the Republican party" (p. 93). Right and Left on foreign policy issues do not often coincide with Right and Left on domestic issues. Finally, Margulies believes that, "had the Senate approved the treaty, the process by which Americans adjusted to twentieth-century international realities probably would have been significantly accelerated" (p. 273). Maybe I am too skeptical, but I can readily imagine the Congresses of the mid-1930s (the Congresses that enacted the neutrality legislation that was so antipathetic to the whole idea of collective security) forcing a U.S. withdrawal from the League as well.

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LOUISE M. YOUNG. *In the Public Interest: The League of Women Voters, 1920–1970*. Foreword by PERCY MAXIM LEE. Assisted by RALPH A. YOUNG, JR. (Contributions in American Studies, number 96.) New York: Greenwood. 1989. Pp. xiii, 199. \$39.95.

In the preface to her book, Louise M. Young explains that she embarked on this project in response to a request from a former president of the League of Women Voters, Julia D. Stuart, who was anxious for a history of the organization to celebrate its fiftieth birthday. Young played a major role in the deposition of the league's records in the Manuscript Division of the Library of Congress and was, apparently, familiar with the written primary sources as well as with the history of the members. Because of these affiliations, it is perhaps understandable, but disappointing, that the result is a detailed organizational history void of any analysis of the league in relation to the broader field of political activism; the league as it related to other women's organizations, especially those involving other former suffragists in the 1920s and 1930s and the new feminist movement of the mid-1960s; and relationships among league leaders and between the national office and local chapters.

Up to now, the little that has been written about the league has concentrated on the history of local branches or on the formation of the organization in 1920 out of the vestiges of the National American Woman Suffrage Association (NAWSA). As a vital postsuffrage movement organization, the league was central in focusing women's attention on the issues facing them as new voters. Therefore, a book concentrating on this group's history is quite necessary in order to enhance historians' understanding of politics within the suffrage movement after the vote was won. Where did the women go? Were there any conflicts over what to do with the NAWSA after 1920? Did the militant suffragists of the Congressional Union and National Woman's party try to influence the NAWSA's evolution into the League of Women Voters?

How did the league shape itself in reaction to the post-World War I Red Scare, especially since its founder, Carrie Chapman Catt, was an object of governmental and media harassment during her suffrage years?

An investigation into the league's stands on important controversial issues would also be of great importance to historians. This book covers several key events up to 1970, such as the struggle over the child labor amendment proposed in the 1920s, the advent of World War II, the development of the atomic bomb, the McCarthy era, and the Vietnam War. Once again, however, the coverage is disappointing in its superficiality. Conflict, controversy, and personalities are left out so that the result is more a listing of key votes or events rather than an analysis.

In spite of its lack of depth and bite, the book is still a welcome contribution to the study of women's organizations. It is well documented with wonderfully detailed footnotes, nice photographs, and an extensive bibliography. It offers a survey of the league's activities from 1920 through 1970, which will aid both students and researchers. In other words, this work is, one hopes, just the starting point for more challenging studies of a fascinating organization that has been in the public eye for seventy years.

HARRIET HYMAN ALONSO  
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JOHN R. SCHMIDT. *"The Mayor Who Cleaned Up Chicago": A Political Biography of William E. Dever*. DeKalb: Northern Illinois University Press. 1989. Pp. xvi, 239. \$28.50.

William E. Dever's name is not well known, but a book about him is worthwhile for several reasons. In background, interests, and career development, he is a good example of a Progressive era politician and mayor, and the fact that he served in the 1920s is one indication of the perseverance of Progressive ideas and concerns into that decade. Dever's career also reflects many of the issues that became central in the 1920s, in particular Prohibition.

John R. Schmidt traces Dever's career chronologically from his origins as the son of an Irish Catholic currier and tanner in a small town in Massachusetts. Dever came to Chicago in 1887, continuing to practice his inherited skilled trade while he went to law school at night. As a young lawyer, he became active in both nonpartisan reform groups and the Democratic party and was elected to the city council in 1902. His interests were typically Progressive, ranging from bathhouses and public parks to transit reform, and many of them were frustrated by the highly fractured nature of both parties in the city at the time. He failed in his first efforts to achieve higher office but finally made it to the superior court in 1910.

Intraparty divisions made political progress a chancy business in Chicago during these years, but

Dever successfully negotiated the cross-currents to get the Democratic nomination for mayor in 1923 and to defeat the controversial two-term Republican mayor, William Hale ("Big Bill") Thompson. Unlike later Democratic mayors, Dever was never the party leader (that position remained with George Brennan), so his power was always limited.

As "the mayor who cleaned up Chicago," Dever applied a traditional Progressive approach to urban politics. He made good appointments to office, tried to remove politics from the school board, replaced many patronage positions with civil service ones, and so on. He was also a successful builder but failed in a major effort to reform abuses in the transit system. Dever controlled Chicago's rampant crime wave with some success but also alienated people along the way, particularly African Americans when he had the police bear down on illegal "black-and-tan" nightclubs.

Most controversial, however, was his position on Prohibition. Although always insisting that he opposed Prohibition, Dever nonetheless determined to apply the law, because it was the law and led to so much crime and violence. Even though he continued to argue for an end to Prohibition, his image was one of a Prohibition enforcer, which seriously undermined his popularity in this very wet city and was a key factor in his defeat for reelection in 1927 when Big Bill returned to power on the premise of a "wide open town."

This well-researched and nicely written biography provides a good analysis of both Dever's career and the political context in which he functioned. It reminds us, once again, that biographies, even of relatively minor figures, can be good vehicles for the study of larger issues.

JOHN M. ALLSWANG  
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RICHARD J. ALTENBAUGH. *Education for Struggle: The American Labor Colleges of the 1920s and 1930s*. (Labor and Social Change.) Philadelphia: Temple University Press. 1990. Pp. xi, 339. \$39.95.

Perceiving the onset of a new era in industrial relations in the aftermath of World War I, progressives inside and outside the labor movement directed their attention to education as an instrument for the social emancipation of the working class. Believing that the formal education system at all levels encouraged submissive behavior and fostered antagonism to labor, these progressives determined to build schools that would instill dignity and confidence in working people and advance the cause of labor. The workers' education movement, which attained peak influence in the early 1920s, involved a wide range of projects, including night schools sponsored by unions, university-affiliated institutes, and independent residential

colleges. Richard J. Altenbaugh has contributed a valuable study of this last approach, focusing on three major residential labor colleges: Brookwood Labor College (1921–37) in Katonah, New York, by far the most important in its impact on the labor movement; Commonwealth College (1923–41) near Mena, Arkansas; and Work People's College (1904–41) in Duluth, Minnesota.

Altenbaugh briefly traces the emergence of American workers' education from early prewar efforts by socialists (including the Finns who established Work People's College) through the 1920s, when many unions and universities became involved, with some three hundred projects undertaken. There is some useful information in this section, but Altenbaugh does not adequately explain why workers' education assumed such importance after World War I. He should, for example, have examined the significance of the postwar mood of "reconstruction" and the new possibilities for labor that it assumed, as well as the impact of the 1919 strike wave. The author also should have provided a fuller explanation of labor's disenchantment with public education, focusing on labor's concern over the rise of "tracking" and the content of social studies courses.

The greater part of the book consists of a thorough examination of the social and educational goals, curricula, internal conflicts, and decline of the independent labor colleges. Altenbaugh describes the social studies-oriented curricula and experiments in "proletarian drama" designed to stimulate militancy. Teachers sharply criticized the conservative craft unionism of the American Federation of Labor (AFL). Altenbaugh shows how the schools' daily routine and administration reflected their commitment to a more democratic and cooperative society. Students and faculty shared work tasks and decision making.

Another strength of the book is the careful attention given to the campaigns against the schools, mostly from the Right. Commonwealth College, badly scarred by American Legion red baiting in 1926, faced serious harassment from planters and their government allies after it developed ties with the Southern Tenant Farmers' Union in the 1930s. The AFL openly denounced Brookwood in 1928 and urged its affiliates to withdraw any support. The school nevertheless survived for many years, during which it trained many key activists in the Congress of Industrial Organizations and in the sit-down strikes.

Altenbaugh argues that only the independent labor colleges could adequately train workers for service in the labor movement; projects affiliated with unions or universities could not sustain radical or even liberal goals. But he devotes little attention to these latter projects, which often varied widely in approach. Several Brookwood faculty members also taught in union and university-sponsored projects; Altenbaugh might have examined how they compared their experiences. He might have considered whether the

independent schools were as sensitive to the needs of women workers as the Bryn Mawr Summer School for Women Workers. The independent school, geared for a small number who could stay a year or more, can also be seen as a retreat from the broader workers' education goal of establishing an "educational highway" to benefit the masses of working people, best accomplished through night classes. The Boston Trade Union College (1919–31), a leading union-sponsored program overlooked by Altenbaugh, had several radical teachers, allowed students a major role in administration, reached many more workers than the independent colleges, and, except for Brookwood, had more of an impact on the labor movement.

STEPHEN H. NORWOOD  
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MARGO HORN. *Before It's Too Late: The Child Guidance Movement in the United States, 1922–1945*. (American Civilization.) Philadelphia: Temple University Press. 1989. Pp. xii, 224. \$34.95.

Discussions regarding the appropriate response to delinquency and other behavioral problems of children often occur without historical perspective. Margo Horn's book focuses on the child guidance movement in the United States in the period from 1922 to 1945. The prime mover behind the development of these guidance clinics was the Commonwealth Fund. Relying extensively on the archival materials of the Commonwealth Fund, Horn devotes attention to two important stages of this movement. The first, the Commonwealth Fund's Program for the Prevention of Delinquency (1922–1927), initiated a broad-based community effort linking schools, juvenile courts, and social agencies in an effort to provide direct services to children. This stage led to a series of demonstration clinics established across the United States. The second stage, the Revised Program in Mental Hygiene for Children (1927–33), shifted attention to the development of child guidance as a professional field through the training of psychiatrists, psychologists, and psychiatric social workers to staff these clinics.

Horn's thesis is thought provoking. Specifically, she argues that "the foundation's rapid shift from juvenile crime to the mental health of children resulted from the [Commonwealth] fund's definition of its proper role in society as well as the emerging professional interests of psychiatrists" (p. 10). Furthermore, Horn contends that psychiatrists used delinquency to illustrate the usefulness of psychiatry in attacking a social problem but that the long-term interests of psychiatrists were in treating less severe mental health problems of children, using the clinics as the "professional niche" (p. 23). The focus of the revised program established between 1927 and 1933 shifted attention to a new category—"the problem child"—



children with mild behavioral and emotional problems. At the same time, Horn contends that the Commonwealth Fund played the role of "conservative innovator" (p. 56) and that the foundation's conservatism meshed well with the professional interests of psychiatrists (pp. 53–54).

The major weakness of this book is the data analysis found in chapter 9. Horn presents empirical evidence drawn from 179 case records of only one clinic to support her thesis. The problem is that this strategy ignores the considerable variation across the clinics (especially with respect to the major sources of referrals), which Horn herself notes (pp. 59–64). Her study is also hampered by the small number of cases available for trend analysis, which is central to her thesis. And, finally, some of the data presented are contrary to her thesis. For instance, the data show little change in the proportion of middle-class families using the clinics over the period from 1925 to 1944 (p. 171), which contradicts her argument that "the families who came to the clinic were increasingly middle class by the late 1930's" (p. 175). Similarly, I see little evidence to support her contentions regarding shifts in the sources of referrals (see pp. 161, 175).

On the other hand, the strength of this book is found in the detailed examination of professionalization. Particularly noteworthy are the concerns of professionals about status, especially in relation to gender, class, and religion, and the role of the Commonwealth Fund as "gatekeeper" to the field (see chaps. 6, 7). Horn's analysis reveals the role (and power) of developing professions in conjunction with private foundations in shaping the nature and response to various social problems.

JOHN H. LAUB  
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J. L. HEILBRON and ROBERT W. SEIDEL. *Lawrence and His Laboratory: A History of the Lawrence Berkeley Laboratory. Volume 1.* (California Studies in the History of Science.) Berkeley and Los Angeles: University of California Press. 1989. Pp. xxvii, 586. \$29.95.

Berkeley, California, welcomed Ernest Orlando Lawrence in 1928, the summer he turned twenty-seven, already three years past his Ph.D.—two as National Research Fellow, one as assistant professor, all at Yale—and reputed a brilliant experimentalist. Having lost a bid for his services the year before, Berkeley sweetened its offer: Lawrence came to California a very well-paid associate professor with very light teaching duties. His brief was research, the payoff spectacular. Within little more than a decade he invented the cyclotron, created the Radiation Laboratory, revolutionized experimental nuclear physics, achieved world fame, won a Nobel Prize, and transformed the conduct of science. This book, the first of three planned volumes, tells how it happened.

Volume 2 will cover the war years from Pearl Harbor through Korea; volume 3, the evolution of the modern laboratory.

Meanwhile, volume 1 offers a ten-chapter narrative history of the laboratory's creation and early career. The opening chapters set the stage. Chapter 1 traces physics research in California during and after World War I and brings Lawrence to Berkeley. The second chapter explores the context and sources of Lawrence's great invention of 1930, formally the magnetic resonance accelerator but better known as the cyclotron (a nickname made official in 1936). The bulk of the book recounts Lawrence's Berkeley career, the further development of accelerator technology, and the laboratory's evolving research program to the eve of U.S. entry into World War II. Among its major themes are institution building and team research; technological development and instrumentation in the conduct of science; scientific entrepreneurship and the financing of big science; cyclotronics as art, science, and social construction; and the transfer of cyclotron technology and products to other laboratories in the United States and abroad. The final chapter follows the transition of Lawrence's laboratory from peace to war in 1940 and 1941.

J. L. Heilbron and Robert W. Seidel bring exceptional qualifications to their project. Heilbron holds a chair at Berkeley in history of science, specializing in the history of recent physics. Seidel wrote his Ph.D. dissertation at Berkeley on the rise of physics in California; since 1985 he has served as administrator of the Bradbury Science Museum, Los Alamos. Both men have amply demonstrated their diligence as researchers and skill as writers. Here they rely chiefly on archival research and contemporary scientific journals, turning to secondary works where necessary, chiefly for peripheral issues, and resorting to oral history only when all else fails, a rare occurrence with such rich archives. The product is masterful, a treasure house of information and insights about the sociology, economics, politics, and philosophy, as well as history, of a major scientific-technical development, presented in lively and even witty prose. Many readers, I suspect, will find the technical discussion in several chapters daunting, despite the authors' notable efforts to render the material accessible. That cannot be helped, as they rightly note, because science and technology are crucial to the story. But readers who persist will be well rewarded and doubtless as eager as I for the sequels.

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THOMAS KESSNER. *Fiorello H. La Guardia and the Making of Modern New York.* New York: McGraw-Hill. 1989. Pp. xvii, 700. \$24.95.

Thomas Kessner has written a rare scholarly work that is also graced by sparkling prose and humor. It should appeal to both professional historians and a wider lay readership.

Kessner has done impressive research in primary sources found in such places as the Charles C. Burlingham papers at Harvard; the Franklin D. Roosevelt papers at Hyde Park; the New York City Municipal Archives; and the Fiorello H. La Guardia Archives at La Guardia Community College, where Kessner served as the first director. He uses oral interviews from the Columbia University Oral History Collection and from a number that he conducted. In addition, Kessner has read widely in the enormous secondary literature on La Guardia and New York.

If Kessner believes that an earlier author has offered a thorough, persuasive analysis of an issue or event, then he incorporates the findings into his own book and acknowledges the source. For instance, Kessner commends Arthur Mann's *La Guardia Comes to Power: 1933* (1965) as "the best analysis of the election" (p. 253) and adds little to it. On the other hand, although Kessner mines the rich resources in Robert A. Caro's *The Power Broker: Robert Moses and the Fall of New York* (1974), he differs from Caro's assessment of the stormy relationship between Robert Moses and La Guardia. Kessner believes that La Guardia generally kept the arrogant, power-hungry Moses under control and harnessed the man's genius for creating public works for the benefit of all New Yorkers.

Kessner's book is an admiring biography of the complex "Little Flower." He credits La Guardia with transforming the bankrupt, corrupt, backward, and depression-ridden city he inherited from the Tammany crowd. La Guardia shaped a municipal government that cared about the "little guy" and attracted people with heart, integrity, and the ability to run the city. He obtained a new city charter and modernized municipal government. Taking full advantage of the unemployment crisis in the 1930s and his special relationship with a sympathetic Roosevelt, La Guardia garnered millions of federal dollars to put jobless New Yorkers to work building an infrastructure of new bridges, highways, parks, playgrounds, tunnels, public housing, and an airport. As head of the National Conference of Mayors and New York's chief executive, he helped to forge a new cooperative relationship between the national government and its cities.

Yet Kessner does not ignore the darker side of La Guardia. The mayor often treated loyal, hard-working subordinates badly, and at times he trampled over civil liberties. In the vain hope of succeeding Roosevelt in the White House after 1940, La Guardia compromised with Tammany, even naming the disgraced former mayor James Walker to a municipal post. Kessner demonstrates that the third term was the least successful. By then La Guardia no longer enjoyed being mayor. He dissipated his energies to no

avail by chasing after a cabinet post, a generalship, and a high diplomatic appointment. His neglected city in the meantime began its long slide toward economic insolvency and an undermaintained, crumbling infrastructure. La Guardia did not tell New Yorkers frankly that the honest, efficient municipal government could not cut costs enough to continue the level of services its residents wanted and that they had to pay more in taxes. Instead, during his third term, his administration began the budgetary sleights of hand that masked deficits, a practice escalated by successive mayors to the point of disaster.

Despite all of La Guardia's shortcomings, Kessner concludes that the mayor "built modern New York, provided relief in heroic proportions, and clasped his city to Washington in a way that changed the history of American cities forever." And, in the words of President Harry S. Truman, La Guardia was "as incorruptible as the sun" (pp. 593-94).

BARBARA BLUMBERG  
Pace University

MICHAEL L. KURTZ and MORGAN D. PEOPLES. *Earl K. Long: The Saga of Uncle Earl and Louisiana Politics*. (Southern Biography Series.) Baton Rouge: Louisiana State University Press. 1990. Pp. xvi, 312. \$24.95.

"Got to clean them choppers," Earl K. Long would say as he dropped his false teeth into a water glass (p. 30). Dining with the governor of Louisiana was always a memorable experience. Indeed, it is a rare Louisianian who does not have an "Uncle Earl" story. Such tales tell of his skulduggery, his vaulting crudeness, and his epic fornication in the fleshpots of New Orleans. In 1959 Long went theatrically insane, and force was needed to extract him from the governor's mansion. Most written accounts of his life have portrayed Long as a pathetically comic figure, and his bumptious amour with the stripper Blaze Starr has been the subject of a recent motion picture. In this fascinating new book, Michael L. Kurtz and Morgan D. Peoples offer the first scholarly biography of Earl Long. They argue that Long suffered from bipolar disorder, or what is popularly called manic depression. More important, they make a strong case that in many ways he was the best governor Louisiana has ever known.

Had Earl not been the brother of Huey Long, it is possible he would not have entered politics. But when Huey was assassinated in 1935, Louisiana soon elected Earl lieutenant governor. Later he served as governor from 1939 to 1940, from 1948 to 1952, and from 1956 to 1960. His periodic returns to office were occasions for rejoicing in many quarters. "Earl . . . let me open up the cathouses again," one sheriff rapturously declared (p. 180).

But, when Long was governor, there was also rejoicing among Louisiana's poor. He heavily taxed the state's oil and gas corporations and drove through

the legislature a program of luminous reform. Long established charity hospitals and offered the poor free medical prescriptions and dental care. Louisiana led the South in welfare benefits and led the nation in old age pensions. He built a thousand schools, provided free lunches, and increased teacher salaries without regard to race. Black illiteracy dropped by 50 percent, and for the first time a public university in the lower South was completely integrated. More than a decade before the Voting Rights Act became law, Long allowed tens of thousands of blacks to register. At the time of his death in 1960, he was virtually the only important southern politician to have defended the right of blacks to vote.

In light of such a record, it is tempting to think of Long as a kind of uncouth Robin Hood. But Kurtz and Peoples refuse to romanticize his career. Long worked closely with mobsters, protecting their casinos and brothels. And, for all of his service to blacks, he remained a racist. Still, this book argues convincingly that Long was sincerely moved by the poverty of black people. Such a contradiction hurried him on to his breakdown. By May 1959 the White Citizens Council was vilifying him. In the blackest hour of his life, Long went before the legislature. There he raged against his tormentors, uttering words that would help send him to the insane asylum: "You got to recognize that niggers is human beings!" (p. 217).

BARTON SHAW  
Cedar Crest College

GERALD SORIN. *The Nurturing Neighborhood: The Brownsville Boys Club and Jewish Community in Urban America, 1940-1990*. (The American Social Experience Series, number 15.) New York: New York University Press. 1990. Pp. xiv, 255. \$35.00.

In March 1940, the New York City Board of Education denied boys over fourteen the right to use school gymnasiums after hours. In Brownsville, a working- and lower middle-class Jewish community in East Brooklyn, the boys who had been using the gym for pickup basketball games organized a petition drive to have it reopened. They succeeded and then, rather than disbanding, solidified their newly formed organization by continuing to collect dues and by electing officers and recruiting new boys from street gangs and clubs throughout the larger neighborhood.

The Brownsville Boys Club, organized around sports and run entirely by the boys for the first five years, became a true mutual aid society, providing its members with a variety of services. It negotiated for space and free equipment for baseball and basketball leagues, recruited local children for "fresh air" camps, organized trips to Ebbets Field and museums, raised money for the war effort, and even intervened with the police and the parole board to "save" neighborhood boys in danger of going bad.

After the war, the club accepted the help of adult

businessmen and politicians, including future city council and borough president Abe Stark, who raised money for paid staff and eventually erected a million-dollar clubhouse. The financial assistance, of course, came with strings attached, and the boys' club eventually gave way to a professionally staffed, adult-administered social service agency.

In the early 1950s, when the adults took charge, the Brownsville community was still predominantly Jewish but already changing with the outmigration of "whites" and an influx of African Americans and Hispanics. The Brownsville Jewish youth who remained active in the organization, some as paid or volunteer workers, tried to offer the boys they served more than an opportunity to play ball, but they were thwarted by Stark who feared the possible repercussions of the boys' leftist orientation and "their aggressive integrationist direction and involvement in community organizing" (p. 147). In 1953, Stark and the adult trustees effectively ended the life of the Brownsville Boys Club by donating its clubhouse to the city and shifting the administration of all activities to the parks department. In the final chapters of his book, Gerald Sorin describes the transformation of Brownsville in the 1950s and 1960s to a lower-class black and Hispanic community. By 1963, no former boys club member remained in Brownsville, although several continued to live and prosper in the larger metropolitan community.

Sorin attributes the boys' enormous success, personally and collectively, to their Jewish-American ethnic culture with its orientation toward achievement, its emphasis on "ethical obligation," (p. 203), its commitment to community, and its traditions and institutions of charity and mutual aid. To better make this point, he compares Jewish-American culture to other ethnic cultures. This is the least successful part of his study. The portrait that emerges of non-Jewish ethnic cultures, particularly the Italian-American, is one-dimensional and at times rather clichéd.

Nevertheless, though unrelenting in pushing home his thesis, Sorin is quite persuasive in arguing for "ethnic culture" as a "very useful category of historical analysis" (p. 5). He has succeeded admirably in providing his readers with a carefully researched and beautifully written portrait of a remarkable group of Jewish-American teenagers and the institution they founded and nurtured.

DAVID NASAW  
College of Staten Island,  
City University of New York

ROBERT A. CARO. *The Years of Lyndon Johnson*. Volume 2, *Means of Ascent*. New York: Alfred A. Knopf. 1990. Pp. xxxiv, 506. \$24.95.

If it is true that biographers come either to love or to hate their subjects, Robert A. Caro has come to hate Lyndon Johnson. Caro says that, despite the bright

threads in the tapestry of Johnson's life, "there runs a thread much darker." It is a thread of "deceit, deception, and betrayal" (pp. xxvii-xxviii). Indeed, this volume, the second by Caro on Johnson, deals with a period in Johnson's life in which there are no bright threads at all. It is a seven-year period beginning in 1941 during which Johnson was halted in his quest for power because he had become just another member of Congress stymied by the seniority system. Caro maintains that Johnson lost interest in legislative activities and turned to making money corruptly through the radio station in Austin that he acquired with the help of Texas millionaire backers. According to Caro, Johnson's interest in politics revived only when he entered the race for senator in 1948.

Caro's pattern is quite simple. Everything Johnson did or said must be twisted to fit the profile that Caro has in mind. He sees Johnson as a ruthless automaton, driven by lust for power and a need to make others subservient to him. What might be seen in Johnson as ambition, Caro attributes to fear of failure, insecurity, and desperation. Words such as "greed," "corruption," "liar," "deceit," and "coward" come easily to Caro in describing Johnson. When Johnson tells a friend he wants to be president someday, he does not simply say it; he snarls it. When Johnson says or does something for the common people of his district that might have stemmed from his own impoverished background, Caro sees his actions only as another calculated step in his quest for power.

The matter of Johnson's personal courage illustrates Caro's tilted view. As a boy and young man, Johnson refused to take unnecessary risks or to engage in fist fights. For Caro, this made Johnson a "physical coward" throughout his life (p. 37). When the United States entered World War II, Johnson, a young congressman of fighting age, talked big about getting into combat but quietly sought a cushy job as a naval officer inspecting shipyards. In order to get a taste of combat, Johnson got himself sent to the South Pacific where he volunteered as an observer on an extremely dangerous bomber raid on a Japanese base. When the bomber in which Johnson was riding was attacked by Zeros (the one he was originally assigned was shot down in the ocean and the crew eaten by sharks), Johnson remained calm and even got into the navigator's bubble where he could better see the attacking Zeros. For this mission, Johnson received the Silver Star from General Douglas MacArthur. But Caro interprets the whole thing as a political gambit because Johnson later used the experience and the medal in campaigns. Caro also condemns Johnson as a liar for exaggerating the story, especially the number of missions flown, in later years.

But if the threads of Johnson's life are all dark, they are all bright for Coke Stevenson, Johnson's opponent in the 1948 senatorial race. It may come as a shock to some Texas historians that the ascetic, ultra-

conservative, and provincial Stevenson is practically deified as a cowboy-knight, the very embodiment of Texas virtues who sought only to bring honest, economical government to the people of Texas. Caro devotes an entire chapter to Stevenson and thirteen pictures (compared to nineteen of Johnson). Such attention to Stevenson is inexplicable except as a way to show by contrast the monumental injustice of Johnson's theft of the election from Stevenson.

The achievements of Johnson in the 1948 election are viewed with great cynicism. Johnson's come-from-behind victory against Stevenson is negated by the corrupt voting in Duval County. His courageous fight against painful and life-threatening illness is attributed to his fear of losing and becoming a nobody. The fact that Johnson invented a new form of whirlwind, statewide campaigning using big money and the communications media is deplored because it violated the traditions of Texas politics.

There is a vast amount of personal detail in this book, perhaps too much. The work cries out for more judicious editing. But the main problem is that Caro's prejudice shows through in every paragraph. The reader gets so engrossed (or annoyed) by the bias that the big picture is lost. The book may be a huge commercial success, but, as a detached biography of an important historical figure, it is a failure.

DAVID E. CONRAD  
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CLAYTON R. KOPPEL and GREGORY D. BLACK. *Hollywood Goes to War: How Politics, Profits, and Propaganda Shaped World War II Movies*. New York: Free Press. 1989. Pp. x, 374. \$22.50.

During the Second World War, American officials worried that sectors of the public might prove less than supportive of administration plans. The unity and fighting spirit aroused by the Pearl Harbor attack was for many Americans short-lived and misdirected. They distrusted the nation's allies, were unhappy with the Hitler First strategy, hostile toward domestic controls, and suspicious of rumored plans for an increased U.S. role in postwar world affairs. Many were overconfident, quite a few defeatist. Propaganda seemed to offer a remedy, and motion pictures, at the height of their popular appeal, appeared to be the ideal medium.

The task of salting feature films with the government's messages was undertaken by the Motion Picture Bureau of the Office of War Information (OWI). Industry leaders at first resisted the propagandists' attempts at persuasion, worried that their messages would jeopardize the movies' entertainment value. But OWI officials were in a position to determine which films were suitable for export to overseas areas not under Axis control. As allied territory expanded,



the studios, eager to regain their foreign markets, warmed to OWI advice.

This thoroughly researched and well-written account introduces readers to the movie industry and the propagandists, takes us through their encounters film by film, and thoughtfully assesses the result. Clayton R. Koppes's and Gregory D. Black's analysis indicates that Hollywood's penchant for oversimplification and OWI's combination of expedience and idealism produced films that did little to broaden the public's understanding of basic issues. Fascism, the character of the enemy nations, the origins of the conflict, and the nature of war were grossly oversimplified and distorted. Despite OWI objections, movies consistently portrayed the Japanese as inhuman. On the other hand, at the agency's insistence, the Allies were given a noble gloss that hid a tarnished reality. At the same time, domestic issues were deliberately overlooked or neglected, and films perpetuated the prewar stereotypes of blacks and women. The movies thus joined other media of mass culture in suggesting that the war was merely an uncomfortable prelude to perpetual postwar personal happiness for the American people.

Koppes and Black provide an excellent description and analysis of these developments. But they neglect the issue of how well the movie-propagandist collaboration served administration needs. Instead, they speculate on how wartime movies may have shaped the American psyche and contributed to the cold war and lament that movies "obscured the sometimes questionable tactics the U.S. and its allies used to fight the war" (p. 325). But is it realistic to assume that producers and propagandists safely ensconced in Hollywood were aware of Allied wrongdoing or to expect them to have ventured criticism had they been aware? How would the public have responded to pictures denigrating the American fighting man? In the same way, the authors' criticism of the movies' failure to prepare the American public for the complex realities of the postwar world rings a bit hollow. A proper context is absent. Koppes and Black fail to assess the plausibility of film makers or propagandists drawing "realistic" but potentially divisive pictures of the Soviet Union and postwar problems. Could the movie makers and propagandists speak with uniformity, much less authority, on these "big" issues—matters that Franklin Roosevelt chose, with good reason, to ignore?

Despite these reservations, this is an interesting, sometimes engrossing, account of a significant aspect of motion picture history and a valuable contribution to our understanding of America during World War II.

RICHARD W. STEELE  
San Diego State University

AMY KESSELMAN. *Fleeting Opportunities: Women Shipyard Workers in Portland and Vancouver during World War II*

and *Reconversion*. (SUNY Series in American Labor History.) Albany: State University of New York Press. 1990. Pp. xii, 192.

Amy Kesselman's book tells the story of women workers employed in the shipbuilding industry in Portland and Vancouver during World War II. At the heart of Kesselman's narrative are the voices of the women workers, their memories preserved through oral history interviews conducted by the author and other members of the Northwest Women's History Project. The women's voices provide clear and moving access to their own understandings of the experiences documented in government, industry, and union surveys, statistical studies, and wartime publications.

Kesselman's shipbuilders add support to the emerging interpretation of women's wartime experience in defense industries as one of a shift to better paid and previously male-defined employment rather than a shift to employment per se. Contrary to the widely propagated view at the time, most defense workers did not step out of house dresses and into coveralls. Rather, they moved from retail sales, clerical, waitressing, and domestic service to wartime industrial work that could double or triple their salaries. And, for the welders, ship fitters, and electricians of the shipbuilding industry, job satisfaction often increased as well.

In two chapters of compelling detail Kesselman examines "life in the yards" and "welding the seams of the double day." Her account makes clear that although women made up over a fourth of the work force in some shipbuilding factories, most of them worked in sex-segregated conditions. Where they did work alongside men, they faced considerable disapproval of their presence and unwritten rules that prevented them from assuming supervisory positions. The author's analysis of coverage of women workers in *Bo's'n's Whistle*, a Kaiser shipyard publication, reveals an interesting transition from an early depiction of women as bumbling and inept workers to a later depiction as sexy distractions taking men's minds off their work. Kesselman joins other feminist theorists in cautioning against a too simplistic approach to gender in the workplace. Her work suggests that shipbuilding held gendered meaning for male workers, affirming their masculinity as well as their skill. When women proved they could do the work, men found them unsatisfactory for other reasons. The shipyards provide one of the most interesting case studies in the United States of the recognition that women worked a double day. Whereas most wartime industries did little to provide supplementary services for women, shipbuilding in Portland and Vancouver was different. The Kaiser Corporation's child care centers stand as models of what industry could do to transform the workplace for women workers with the aid of wartime government subsidies. Open twenty-four hours a day and staffed by

leading child development experts, the Kaiser centers attracted nationwide attention and convinced many initially reluctant mothers of the advantages of factory-located group care.

This book is a welcome addition to the growing body of literature on the experience of working women during World War II. Kesselman's model monograph is clear, precise, and well written and adds personal meaning and interpretation through the voices of her narrators.

JOYCE SHAW PETERSON  
Florida International University

PETER J. T. MORRIS. *The American Synthetic Rubber Research Program*. (The Chemical Sciences in Society Series.) Philadelphia: University of Pennsylvania Press. 1989. Pp. xii, 191. \$34.95.

In an age when historians have become ever less venturesome in attempting the "big picture" and the grand hypothesis, Peter J. T. Morris's study of American rubber research makes him in some respects an Icarus among the earthbound. Like the nineteenth-century positivists, Morris asks the historical record to do no less than yield lessons—to guide the makers of national research policy. The problem at hand is Japan's growing "high tech" superiority, which has multiplied calls for government-sponsored collaboration with industry and academe to enhance American competitiveness.

But will such programs do the job, that is, produce the sort of radical innovation that the United States needs to reclaim its lead? The answer, Morris believes, lies in the history of the government's collaborative rubber research program of 1942–56, a project born out of the pressing wartime need for synthetic rubber. Morris here is both cautious and assured: "It is perhaps foolhardy to generalize from the history of one project to other quite different projects," yet the "rubber research program can still offer important indicators to the best way to fund research and promote technological innovation" (p. 2).

What do those "indicators" indicate? Principally Morris finds that government-funded collaborative projects do not work—or not as well as desired. True, the wartime phase of the project produced large quantities of the needed synthetic, but after 1945, to quote Morris's experts, it "achieved only mundane results" (p. 142). What, then, does work? In words that will warm the hearts of "free marketeers," Morris concludes that "radical innovation is best promoted by competition," especially commercial competition, which "remains the best means of accelerating technological innovation in a free society" (p. 142).

A grand hypothesis, but I am not convinced. As Morris himself recognizes, one cannot after all draw lessons from one example (if at all). Moreover, demonstrating that a collaborative model had only limited

success (and Morris is best here) does not prove the advantage of the competitive one. In fact, despite his tribute to free enterprise, Morris admits elsewhere that its research record is paltry: since government involvement ended, "there has been only one major technological innovation in the synthetic rubber field" (p. 22).

Besides conceptual flaws, the book is also burdened by its own material. James Joyce once noted, "People are more interested in other people than anything else." But here people are virtually absent, and leading roles are filled instead by chemical modifiers, initiators, and long-string molecules. Reading about them—for 142 pages—leaves the reader finally groggy from technical overkill. A narrative that informs us, in a typical passage, that the "size of a polyvinyl chloride molecule in solution lay between the extremes of a random coil with free bond rotation and a rigid rod" (p. 85) may make for a good laboratory report, but it does not make good history. As a result, the book's audience will be mainly polymer chemists and rubber scientists. As for those research policy formulators, they will find useful insights, but they will hunt in vain for lessons.

E. H. BEARDSLEY  
University of South Carolina

MAX HOLLAND. *When the Machine Stopped: A Cautionary Tale from Industrial America*. Boston: Harvard Business School Press. 1989. Pp. xiii, 335. \$22.95.

Max Holland presents a fascinating account of the post-World War II rise and decline of one of America's leading companies in the machine tool industry, Burg Tool of Los Angeles. Founded in 1946 by an inventive machinist, Fred Burg, the company enjoyed rapid growth for about twenty years as a personally run family business producing high-tech machine tools for niche markets. Innovation, service, and the possession of specialty products combined with the existence of a favorable external environment (such as defense spending for the Korean War) to bring success to Burg Tool. Perhaps most important, Fred Burg and others in management cooperated closely with their workers constantly to improve the company's products and production processes; they were not afraid of getting their hands dirty on the factory floor. This approach to business changed, Holland shows, after Burg Tool was acquired by the conglomerate Houdaille in 1965. From that date on, Burg Tool's course was downhill. The most basic change was managerial, as Houdaille executives who knew little about machine tools took over the management of the company. Assuming a short-term approach to business, they milked Burg Tool for immediate profits at the expense of long-term investments in research and development. As a consequence, Burg Tool lost its innovative edge, proved unable to meet growing competition from Japanese machine tool-

makers, and went out of existence in 1986. Holland clearly demonstrates that its own mistakes, not simply Japanese competition or the reluctance of politicians to provide tariff protection from foreign imports, were what killed Burg Tool.

Based on research in Burg Tool's records, oral history interviews, and other primary sources, this case study offers, as its subtitle suggests, provocative insights into some of the problems with international competition that U.S. industrial enterprises have recently faced. Throughout his work Holland connects the evolution of Burg Tool to general developments in U.S. and global business. Logically organized and sprightly written, Holland's history works well in the classroom. I have successfully used this study—now available in paperback—as required reading in both graduate and undergraduate courses in U.S. and comparative business history and strongly endorse its adoption for students.

MANSEL BLACKFORD  
Ohio State University

MICHAEL WALA. *Winning the Peace: Amerikanische Außenpolitik und der Council on Foreign Relations, 1945–1950*. (Studien zur modernen Geschichte, number 41.) Stuttgart: Franz Steiner. 1990. Pp. 331. DM 68.

In this excellent book on the Council on Foreign Relations, Michael Wala examines the origins of this New York-based organization between the world wars and then concentrates on its role during Harry S. Truman's presidency from 1945 to 1950. For those formative cold war years, when the United States was establishing itself as a global power, Wala evaluates the council's discussion and study groups and its impact on policy making, especially through shaping public opinion.

In his treatment of its early history, Wala covers much of the same ground as Robert D. Schulzinger's *The Wise Men of Foreign Affairs* (1984), the outstanding book on the Council on Foreign Relations. He reviews the efforts of the lawyers, bankers, academics, and others who founded the council to foster "internationalism" in the tradition of Woodrow Wilson but with sufficient distance from the League of Nations to attract Republicans such as Elihu Root. He notes as well the council's work with the State Department to prepare for war and plan for peace during World War II, particularly with the War and Peace Studies.

But for the cold war years from 1945 to 1950, Wala goes far beyond Schulzinger in the depth of his research and in his analysis of the Truman administration's policy making. He also offers a different interpretation. Rather than dismissing the council's contribution to American foreign policy as fairly insignificant, as Schulzinger did, Wala argues that its members played key roles in fostering a bipartisan foreign policy. He makes this point most convincingly

in connection with the Marshall Plan, which Schulzinger largely ignored. He reveals the complex interconnections between officials in the Truman administration and members of the council, which allowed those members to have private access to the policy makers and the policy makers to have opportunities to disseminate their ideas. The kind of relationship that led George F. Kennan to publish the "X" article in *Foreign Affairs* in 1947 continued to operate in the development of the Marshall Plan.

So successful were these relationships, Wala concludes, that together the Truman administration and the council were able to create the "illusion" (p. 243) of grass-roots support for the Marshall Plan and thereby ensure congressional funding of the European Recovery Program. They were able to establish a cosmopolitan or internationalist foreign policy despite the isolationist tendencies of the general public. This successful "manipulation" (p. 250) by the foreign policy establishment leads Wala to conclude that the study of U.S. policy making must include a theoretical model that accounts for the elite's influence. But, he cautions, one should not view this elitist role as a conspiracy, as extremists on the Right and Left have done in condemning the council. Nor should one dismiss the council's importance because of the difficulty of proving its direct influence. Rather, he argues, what was most significant were the premises and the world view that the council members and the Truman administration officials held in common, resulting in a broad consensus that shaped the parameters of U.S. foreign policy in the cold war.

Several German historians have made major contributions to the history of U.S. foreign relations. Wala joins that growing list, which includes Günter Moltmann, Erich Angermann, Klaus Schwabe, Reinhard Doerries, Detlef Junker, and Hans-Jürgen Schroeder. Unfortunately, not all of their books have been translated into English; some have therefore not received the attention they deserve. Like theirs, Wala's book merits serious study on both sides of the Atlantic.

LLOYD E. AMBROSIOUS  
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H. W. BRANDS. *The Specter of Neutralism: The United States and the Emergence of the Third World, 1947–1960*. (Columbia Studies in Contemporary American History.) New York: Oxford University Press. 1989. Pp. vii, 372. \$40.00.

The ideological bipolarization of Europe after World War II served to create an uneasy and precarious balance of power. Seeking to tilt the balance in their own favor, both Soviet and American leaders of this postwar period worked vigorously to entice the non-aligned ("Third World") nations into their respective camps.

In this volume, H. W. Brands examines this diplomatic wooing from a decidedly American perspective. He prudently elects not to trace the entire diplomatic history of U.S. relations with all nonaligned countries. Rather, he selects three key states—Yugoslavia, India, and Egypt—each from a distinct geographic region, for detailed examination in the main divisions of the book. The political leaders of these three nations, Josip Broz Tito, Jawaharlal Nehru, and Gamal Abdel Nasser, were, of course, powerful figures and each a justifiable legend in his own right in the world of diplomacy.

The author presents a refreshingly candid, real-politik interpretation of U.S. relations with the neutral nations. Presidents Truman and Eisenhower (and by extension their secretaries of state, Dean Acheson and John Foster Dulles) failed spectacularly in Egypt, Brands concludes, but in other countries, such as India and Yugoslavia, they did reasonably well when pursuing realistic objectives. Most U.S. policy failures, Brands believes (contrary to revisionist wisdom), arose not so much from “cloudy perceptions” or a “rigid application of anticommunist ideology” (p. 321) as from a conflict between divergent and sincerely held perceptions of national self-interests.

The Truman administration does not receive as much attention from Brands as the Eisenhower administration, but it is sufficient. Eisenhower and Dulles are depicted as occupying a higher moral plane than their predecessors; they believed that a nonaligned stance between “good” (U.S. democracy) and “evil” (Soviet communism) was, in and of itself, immoral in the extreme. But, stretching credulity to the very brink, Brands claims, “Eisenhower and Dulles had lived long enough to know that the arena of military strategy and great-power politics was essentially amoral” (p. 307). Can Brands have it both ways?

The author describes Dulles as an “unbending moralist” who staunchly absorbed the political heat from unhappy conservatives and the “groans from the liberals” for his pious and ambiguous positions, many of which seemed to be at variance with Eisenhower’s expressed views. The most serious threat to the administration’s policies toward the neutral nations came from the gurus of the Far Right fringe, members of Eisenhower’s own party, such as William Knowland and Joseph R. McCarthy. By sacrificing himself before the irrational (as often as not) assaults of such congressional critics, Dulles made it possible for Eisenhower to “climb the high ground of moderation and statesmanship” (p. 307). One hopes Eisenhower would have blushed at such hyperbole.

Petty cavils to the side, this study is diplomatic history at its best: impeccably researched, well written (and edited), thoughtful and provocative.

RICHARD F. HAYNES  
Northeast Louisiana University

BRUCE J. DIERENFIELD. *Keeper of the Rules: Congressman Howard W. Smith of Virginia*. Charlottesville: University Press of Virginia. 1987. Pp. xii, 306. \$30.00.

Bruce J. Dierenfield has written a first-rate study of the influence that the late Congressman Howard W. Smith of Virginia had on the legislative process through his position as chairman of the House Rules Committee in the 1950s and 1960s. Smith was opposed to all manner of legislative support for social change and held racist views toward African Americans and Asians. Although these views were not unique in the House, Smith’s importance is rooted in the ways in which he was able to curtail legislation through his use of the Rules Committee. Chairmanship of the Rules Committee provided him the vehicle to try to hold back the flow of twentieth-century social legislation. So powerful was Smith that even Speaker of the House Sam Rayburn could not deliver on his promise to Democratic liberals for Smith’s cooperation on legislation in the late 1950s. Ultimately, after the election of John Kennedy, the Rules Committee was enlarged to make certain that vital New Frontier laws could get to the House floor for debate. Until his congressional defeat in 1966, however, Smith remained a force.

What does the career of Smith tell us about the Congress? Given Smith’s increasingly outdated political philosophy, how was he able to amass and hold so much power? He did it by using the governing rules of the House of Representatives. Smith was a superbly adroit parliamentarian who took great pains to stay out of the limelight. He also was a tireless worker who devoted most of his life to his political agenda. The disheartening fact is that most of his work was an attempt to obstruct much-needed social legislation. Rather than using legislation to solve the social and economic ills of the United States, Smith sought to preserve his view of nineteenth-century America.

Dierenfield depicts Smith as a man who had little personal life and was driven in his work by his ideology. His basic intelligence, coupled with his talent for administration, led him to his position of power. Unlike Joe McCarthy in the Senate, Smith was not a headline grabber or a person who made a great many personal enemies. Not until enough Democratic liberals became frustrated with his obstructionist use of the Rules Committee chairmanship did his power wane, demonstrating that even the most powerful members of Congress can eventually be thwarted by their colleagues. What strikes one as most interesting is how well entrenched a congressional member holding an antiquated philosophy could be in the early 1960s.

Perhaps this account, by surveying Smith’s career and detailing his skillful manipulation of the political system, will encourage a healthy skepticism about the internal dynamics of Congress. It dramatically demonstrates the effects that a single member can have in



hindering the flow of a significant amount of legislation.

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MICHAEL DAVIDSON. *The San Francisco Renaissance: Poetics and Community at Mid-Century*. (Cambridge Studies in American Literature and Culture.) New York: Cambridge University Press. 1989. Pp. xvi, 248. \$34.50.

Norman Podhoretz called them the "know-nothing bohemians." For Irving Howe, they were "the other side of the American hollow," whose "contempt for mind" mirrored "the middle class suburbia they think they scorn." Even historians more sympathetic to the San Francisco poets of the 1950s have emphasized their energy and style at the expense of the content and aesthetics of their poetry. The San Francisco writers figure in most cultural histories as exotic precursors of the counterculture of the 1960s, flamboyant rebels waiting for a cause.

The virtue of Michael Davidson's study is its careful attention to the complexity and variety of the poetry produced by the San Francisco renaissance. In his view, the Whitmanesque exuberance of an Allen Ginsberg is just one strain of a heterogeneous effort to give voice to communitarian longings while acknowledging the barriers to community in cold war America. By detailing the ways in which these writers grounded their poetics in specific contexts of performance and shared experience, Davidson reveals a plaintive desire for fellowship that belies the usual portrait of the Beats as antinomian individualists. "In the spiritual and political loneliness of America of the fifties," Gary Snyder recalled, "you'd hitch a thousand miles to meet a friend" (p. 23).

Yet Davidson is sensitive to these poets' ambivalence about the possibilities for such friendship and to the inward-looking and repressive features of their actual communities. For women poets, the coterie poetry circles in the Bay Area—especially the one surrounding Robert Duncan, Jack Spicer, and other homosexual writers—were simultaneously liberating and authoritarian. Davidson's best chapter describes how the early feminist poetry of Denise Levertov, Diane DiPrima, Helen Adams, Joanne Kyger, and Judy Grahn drew on the openness of the Beat community while challenging its misogynous "boys' club" atmosphere.

Although this is not a historical study, Davidson shows how the San Francisco renaissance situated itself historically by its elegiac references to previous episodes of cultural and political radicalism, particularly West Coast anarchism and sexual radicalism and Transcendentalism. Such "enabling fictions" provided a historical precedent for these poets to read their own personal lives in cultural terms and to

synthesize public and private aspirations in alternative lifestyles. They also validated their "impact in the political arena . . . as a kind of oppositional sign" (p. 27).

Historians will wish that Davidson had said more about the specific institutions and associations that nurtured an alternative culture in San Francisco. Davidson might also have tried to place these poets in a longer tradition of modern cultural radicalism. In 1916–17, the *Seven Arts* anticipated these writers' interest in personal renewal and community as well as their anxieties about the realization of such goals in a bureaucratic age. Dwight Macdonald's *politics*, which published Duncan's important essay on homosexuality in 1944, also expressed similar concerns, while providing a haven for kindred spirits like Paul Goodman. Davidson's nuanced study suggests how much these poets had in common with earlier cultural radicals, finding a commitment to community where others have seen only self-assertion.

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HUGH DAVIS GRAHAM. *The Civil Rights Era: Origins and Development of National Policy, 1960–1972*. New York: Oxford University Press. 1990. Pp. x, 578. \$29.95.

Hugh Davis Graham has produced a massive work with a clear thesis. Using the commonplace political rhetoric of the 1980s, he takes aim at those who detract from the stunning success of the civil rights movement of the 1960s by their emphasis on the persistent inequities of race and sex in our society. In addition, instead of finding continuity and progress in the employment of women and minorities in the 1970s, Graham deplores developments after the passage of the Civil Rights Act of 1964 and the Voting Rights Act of 1965. Problems of policy and implementation, he believes, led to a shift in administrative and judicial enforcement; a goal of equal treatment was replaced by a goal of equal results. The hallmarks of this period became preferential treatment, reverse discrimination against white males, and taboo racial and sexual quotas: "This shift produced a stiffening of white resistance that was reflected in the public opinion polls" (p. 456). The civil rights focus fit within a larger shift in the nature of the American administrative state from consolidation to disaggregation and even greater intrusiveness through governmental regulation. This shift saw the creation of new agencies, such as the Environmental Protection Agency and the Equal Employment Opportunities Commission established in the Civil Rights Act of 1964, to protect citizens from harm by private or government behavior.

The narrative painstakingly describes governmental processes and procedures leading to the major civil rights legislation and policy of the 1960s and its

implementation during the Nixon presidency. The author has laboriously mined voluminous files in the three presidential libraries and the National Archives. The work produced makes provocative reading but is methodologically flawed.

Participants in the civil rights movement and governmental policy makers responding to their efforts had different perspectives on their achievements. Graham advances one perspective. In his effort to validate his view, he submerges the fuller picture: the interaction of conflicting groups, personalities, and officials and the dynamics of the policy process—a study of which requires the use of additional materials.

Graham's reliance on the paper morass of government is a weakness when so much other evidence is available for such a recent period. Paper trails are inadequate and often purposely misleading guides to who or how or why or even when any policy decision was made. Many memoranda are written as justification; they may not even be factored into the decision-making process. He uses few interviews, and these are mainly transcribed oral interviews done by others. But there is Nixon White House assistant John Erlichman who not only was interviewed but also gave the chapters on the Nixon administration a "careful reading" (p. viii). Most of the men and women whose actions are discussed at length based only on the paper trails are still alive and quite eager to talk. Many are even listed in the telephone directory. Their recollections deserve to be tested against one another and against the documentary evidence. For the record I should note that I participated in the Nixon administration affirmative action programs and the investigation of allegations of reverse discrimination. I would have gladly given an interview had I been asked.

Graham finds dissonance and diversion in the philosophy and goals of what he calls the black civil rights movement and the women's movement (black women?). But his argument is not supported by any evidence, written or oral, from any civil rights or women's rights leaders. Puzzling also is Graham's suggestion that women's organizations should have endorsed a compromise that would have added prohibition of sexual discrimination to the Fourteenth Amendment as a substitute for an equal rights amendment. Space considerations do not permit a full explanation but the short and logical answer is that such a strategy would have had to negotiate the same ratification process and would have raised the same questions concerning men and women's roles. In addition, the process would have created an opportunity for antagonists to weaken the protection against race discrimination, which no civil rights proponent cared to do.

Graham provides much useful information, along with strong arguments based on "history," for opponents of civil rights in the pre-Ronald Reagan period to attack its supporters. Evidentiary problems, includ-

ing the selective use of secondary materials, the paucity of interviews, and scanty analysis of the coverage of the subject in the major electronic and print media and in materials in nongovernmental archives, make the result a decidedly incomplete though weighty story.

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PATRICK LLOYD HATCHER. *The Suicide of an Elite: American Internationalists and Vietnam*. Stanford: Stanford University Press. 1990. Pp. ix, 429. \$35.00.

Patrick Lloyd Hatcher harrows fields well plowed by Douglas Blaufarb (*The Counterinsurgency Era* [1977]), Leslie Gelb and Richard Betts (*The Irony of Vietnam: The System Worked* [1979]), Larry Berman (*Planning a Tragedy* [1982]), and Larry Cable (*Conflict of Myths* [1986]). To rework the old ground, he fashions a new but not necessarily effective hook.

In his attempt to understand and explicate the intellectual failures that produced the U.S. policy debacle and military defeat in the Vietnam War, Hatcher establishes a multilayered dialectic. He pits Lockean against Leninist, Whig against Tory, and maximalist against minimalist. The Lockean constituted the internationalist cold warriors operating under the imperative of blocking perceived or presumed communist expansion. Whig internationalists gave priority to modernizing political and security structures within countries threatened by Leninist movements, whereas Tory internationalists favored the reform and modernization of economic structures. The tensions between the two priorities had significant policy implications as the United States became increasingly involved in South Vietnam. Maximalists, in Hatcher's view, sought slippery slopes with eager abandon, whereas minimalists constantly saw the limitations of power and capacity that governed interventionist diplomacy.

The tension of the interlocking dichotomies and the triumph of the maximalist Whigs served to lead the Johnson administration through a series of progressively catastrophic escalations of involvement and goals within Vietnam as failure bred not reconsideration but reinforcement. Overall, the theoretical construct makes for interesting argument, if on occasion difficult to follow, and at bottom parallels the conclusions of the earlier writers. Unfortunately, Hatcher severely enervates the power and salience of his interpretation by his inadequate use of the resources at the Lyndon Baines Johnson Library.

Had he employed primary sources to the degree to which he marshalled the secondary literature, he would have discovered that men such as John McNaughton and Robert McNamara, whom Hatcher characterizes as maximalists, saw their military architecture, particularly the air war against the lines of communication in Laos and the infrastructure in

North Vietnam, as minimalist and low-risk actions. He would have discovered that the administration without notable exception saw the ground force commitment and augmentation in 1965 as a minimal, prudent response to a northern escalation. Hatcher would have seen in such files as the Komer-Leonhart papers that the Whig-Tory tension was far more apparent than substantial.

He might even have become convinced that President Johnson, far from being a maximalist from 1966 to 1968, was a force for minimalism in the face of the Joint Chiefs of Staff.

The Vietnam War was a passionate conflict within the United States, and that fog of emotional dispute continues to dog an accurate understanding of American policy and its consequences. An overreliance on secondary works or the memoirs of participants serves to thicken the fog of ambiguity, polemic, and exculpation, which has done so much to retard Vietnam War scholarship. Analyses and interpretations that are not based on the documents, no matter how novel and intellectually intriguing, may serve to divert or pique, but they do not illuminate and explain.

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CHARLES DEBENEDETTI. *An American Ordeal: The Antiwar Movement of the Vietnam Era*. Assisted by CHARLES CHATFIELD. (Syracuse Studies on Peace and Conflict Resolution.) Syracuse: Syracuse University Press. 1990. Pp. xvi, 495. Cloth \$49.50, paper \$16.95.

With considerable success, this study explains three important phenomena: the emergence of an opposition movement to the Vietnam War within the United States, the relation of that movement to changing American popular moods, and the interaction between antiwar dissenters and the U.S. government. Had Charles DeBenedetti lived beyond 1987, he might have added depth to his study by interviews, but deficiency in oral history data is largely compensated for by the thoroughness of archival research. More serious is the book's lack of sustained attention to the Vietnam War, of which the antiwar movement had to be an appendage. The problematic of the movement's relationship to the Indochina conflict itself remains still to be unraveled.

Yet DeBenedetti's work will long be the standard treatment of its subject. He convincingly traces the institutional beginnings of the Vietnam antiwar movement to post-Korean War efforts of pacifists to stimulate opposition to thermonuclear weapons and to their testing. The organizations established at that time comprised the framework for renewed peace activity once the Indochina war burst on popular American consciousness in about 1963 (it had, of course, been a fact of life for the Vietnamese for over two decades). By 1965, however, the new peace

movement outgrew its pacifist origins and, shaped by the political and cultural storms of the 1960s and 1970s, became at least as formless and unruly as the American society in which it operated. The "abiding irony of the antiwar opposition," DeBenedetti wrote, was "that its most assertive advocates were held in . . . contempt . . . by a [U.S.] population that increasingly termed the war a mistake" (p. 284).

The main subject of this study is the efforts of a shifting group of religious and secular militants to direct and sustain and even create an antiwar spirit in the United States. Each year's seemingly inexorable escalation called forth a new set of plans for another spring or fall "action"—local or Washington-based demonstrations, antidraft protests, teach-ins, nonviolent and sometimes bloody confrontations with various levels of authority. Skillfully using materials from the Swarthmore College Peace Collection, the State Historical Society of Wisconsin, and a score of other depositories, as well as relevant printed sources, DeBenedetti goes beyond merely chronicling the year-by-year rhythms of the movement. He offers instead sophisticated analyses of its sometimes clashing tendencies and of strands within the antiwar project, such as the impatience of some militants with the perceived moral "numbness" of the U.S. populace.

He is harsh on the movement's countercultural fringe, some of whose adherents hoped to reproduce in the United States a version of the guerrilla war being waged in Vietnam. But he does not hesitate to reveal the often-unsavory maneuverings of moderate antiwar liberals, who sought both to oppose the Vietnam War and to disavow Vietnamese communism. They also tried to police demonstrations in the United States so as to exclude "extremists," who displayed National Liberation Front flags and dared call for what eventually took place, namely, U.S. withdrawal. The ephemeral nature of much of this activity, its fragmentation resulting from race, class, sectarian, and gender factors, gets careful attention. The author also recognizes the movement's failure to create any lasting organizational structures much less an anti-imperialist party of the Left that could intervene effectively in mainstream American life. (Wholesome experiences of antiwar oppositionists, however, could and did prove useful in later protests against U.S. intervention in the Caribbean, Central America, and the Middle East.)

DeBenedetti's handling of the complex relationship between the antiwar movement and the government, informed by manuscript materials from the Lyndon Baines Johnson Presidential Library, is particularly acute. So, too, is his understanding of ideological diversity in the American Left and of the frustrations engendered by the war's sheer duration (it went on for a half-decade after Richard Nixon's announcement of "Vietnamization" in 1969). He also shows how internal government reports on the movement contradicted the notion that antiwar activity was somehow reducible to the presumably objectionable

category of "Communist inspired or directed." He suggests that such charges, emanating from other governmental and journalistic sources, poisoned the debate on the Vietnam War and helped marginalize the peace movement; they may have even increased the bloodshed in Indochina.

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STANLEY I. KUTLER. *The Wars of Watergate: The Last Crisis of Richard Nixon*. New York: Alfred A. Knopf. 1990. Pp. xiv, 733. \$24.95.

Stanley I. Kutler's ambitious synthesis details the complexities of political sabotage and conspiracies to obstruct justice in evocative contexts including Vietnam and the growth of the imperial presidency. Richard M. Nixon's desire to circumvent opponents was hardly unique, but the scope of his hatred for congressional and other "enemies" was. Nixon is presented as a very sore winner who trusted nobody, delegated nothing, and was a spider at the center of all Watergate webs worth mentioning. Nixon as passive or unknowing victim is nonsense. Nor is Nixon a persecuted symbol of the wider political guilt of the country (and, in particular, of his two immediate Democratic predecessors). Instead, Kutler portrays Nixon as a unique leader who played politics as war and who paid the price for it.

Kutler's approach will hardly enthrall Nixon's defenders. It is not meant to. Watergate is "contested history"; self-serving revisionism and widespread political amnesia are rife. We must grasp Watergate whole, because its implications "go to the heart of our political and constitutional system" (p. xiii). Fittingly, Kutler's work is strongest regarding the varied legal aspects of the case. We can empathize with the very many discomforts of the people peeling the onions of the Watergate cover-ups. We can understand why most legal scholars viewed impeachment as "analogous to nuclear weapons—available but too dangerous to use" for almost two years after the break-in (p. 442). We can feel the drama, frustration, and fear of the judiciary's and Congress's legal disputes, investigations, and final judgments.

Kutler is similarly sharp and unsentimental in his analysis regarding how law translated into politics and vice versa. His discussions of the squelching of the earliest effort to investigate Watergate by Congressman Wright Patman; of the internal tensions within the Senate Watergate Committee; of the cat fights between John Dean, John Mitchell, John Ehrlichman, H. R. Haldeman, and Nixon himself as the initial cover-up stonewall fell apart; and of the debates within the "fragile coalition" of the Republican and Democratic members of the House Judiciary Committee are all dramatically and provocatively presented, as is his spirited defense of the U.S. attorneys who prosecuted the Watergate case before

the appointment of Special Prosecutor Archibald Cox. All of this and more is a model of how to write contemporary history well.

Watergate, however, was a struggle for public opinion, too. Here journalism mattered. Yet, after a very strong early chapter on "media wars" during Nixon's first presidential term, Kutler drops his focus on the news media after the 1972 election. Surely, Watergate became far, far more than Woodward and Bernstein after most of official Washington was drawn into the struggle. But Kutler, perhaps, is a bit too jealous of his analytical status as compared with journalist historians such as Elizabeth Drew.

Kutler must also walk a fine line while writing both for those familiar with Watergate and for those who are not. Usually he proceeds with eloquence and dispatch. But there are points where he depends too little on his normally chronological approach and addresses events in a too extended, nonsequential fashion. The sometimes inevitable back-and-forth involved in his treatments of the types of political sabotage that occurred before the Watergate break-in, the major factors that combined to cause the breakdown of the initial cover-up in March and April of 1973, and the complicated legal, political, and journalistic struggle for the White House tapes throughout early 1974 may confuse younger readers whose time lines about such events are vague at best. Teachers using this book as a text in their classes should plan accordingly.

Overall, however, this study is, and will remain, the standard book on the "underside" of the Nixon presidency for the foreseeable future. It is a worthy example indeed of history made relevant by a skilled craftsman impatient with myth and intolerant of evasion.

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## LATIN AMERICA

OLIVIER DE MONTMOLLIN. *The Archaeology of Political Structure: Settlement Analysis in a Classic Maya Polity*. (New Studies in Archaeology.) New York: Cambridge University Press. 1989. Pp. xvii, 287. \$44.50.

JEFF KARL KOWALSKI. *The House of the Governor: A Maya Palace at Uxmal, Yucatan, Mexico*. (Civilization of the American Indian Series, number 176.) Norman: University of Oklahoma Press. 1987. Pp. xx, 298. \$49.50.

No two works on Maya archaeology could be as different as these. Olivier de Montmollin's book is an anthropological archaeology based on an evolutionary perspective that is almost devoid of historical content and will appeal only to specialists with a particular theoretical bent. Jeff Karl Kowalski, although he focuses on a single structure at a single site, covers a number of historical problems shared by archaeologists and ethnohistorians whether they are



anthropologists, art historians, or historians, and he does so in a manner that will be intelligible to scholars in the various disciplines as well as to students and general readers with an interest in the ancient Maya.

De Montmollin's study of classic Maya settlement "hierarchies" and politics is based on a surface survey of the Rosario Valley in Chiapas, Mexico. The book, however, is written at what the author calls a "middle range" theoretical level, deriving its orientation from the Binfordian processual archaeological tradition. As to readability, it exhibits some of the worst faults of this genre: it is jargon-ridden and written in a "formalistic" style containing many involuted sentences that were frequently unintelligible to me. Its most irritating feature is the author's frequent referral, in abbreviated form, to the numerous "scientific" indexes that he has developed, for example, "TLI" for "Tribute Load Index." Some of the blame, however, for the cumbersome text must be shared by the doctoral committee for which the work was written and especially by the editors of Cambridge's *New Studies in Archaeology Series*.

It is unfortunate that the book is not more readable, for it is not without virtue. I applaud de Montmollin's efforts to integrate the literature on Mesoamerican political structures with that of preindustrial African states, an area with rich ethnographic literature and from which Mesoamericanists can derive useful leads and hypotheses. His "bundled continua" approach to model building and concepts such as centralization, segmentation, integration, stratification, and hierarchy is a great improvement over the simplistic evolutionary models—such as the tribe, chiefdom, state typological scheme—employed by many archaeologists. Although tortuous, his discussions of political anthropological concepts could serve to enlighten many scholars who study archaic societies within an evolutionary processual framework regardless of their specific regional interests.

Of a totally different character is art historian Kowalski's work, which examines the House, or Palace, of the Governors at Uxmal, Yucatán, one of the most famous and finest examples of Puuc-style architecture. His study is based on a thorough review of the extant literature as well as on new field studies. In regard to the palace's physical features, it would seem that no stone has been left unturned.

The book, however, is much broader than its title suggests, for the author examines the wider cultural and historical dimensions of the palace and the site. He also more precisely delimits Puuc chronology (which he argues had its beginnings in the eighth century A.D. and lasted until A.D. 925 or 975), assesses the historical validity of colonial Yucatec sources in relationship to archaeology, examines the relationships of Puuc iconography and iconology to other areas of Mesoamerica, and provides a reading of the hieroglyphic inscriptions at Uxmal and related sites.

One of his most significant conclusions is that the Tutul Xiu, contrary to their claims in such sources as

the Books of Chilam Balam of Mani (Codex Pérez) and the Chilam Balam of Tizimin, did not "found" Uxmal. Instead, they may have used the site primarily as a place of ritual after it had been abandoned, and their foundation claims were politically motivated. In unraveling the complexities of the documentary history of epiclassic and postclassic Yucatán, Kowalski follows Alfred M. Tozzer's and Ralph L. Roys' interpretations of the sources rather than those of Sylvanus G. Morley and Alfred Barrera Vasquez. He also favors the recent archaeological view that the Puuc and "Toltec" phases at Chichén Itzá overlap rather than represent two discontinuous periods; this interpretation also posits an earlier beginning date for the Toltec presence in Yucatán than has been previously held.

In his reading of the texts at Uxmal, Kowalski follows the new approaches to Maya hieroglyphs that have developed over the last two decades and that have illuminated specific details of Maya dynastic history. He identifies one of the pre-Xiu rulers of Uxmal (a Lord Chac) and Uxmal's emblem glyph. His analysis, once inscriptions from other Puuc sites are located and studied, will lead to a greater understanding of the political relationships among Maya cities. One of the great virtues of this work is the many leads it provides for further research.

Well designed with ample illustrations, maps, and indexes and very readable—this volume, too, was a doctoral dissertation—Kowalski's book can be highly recommended both as an introduction to Yucatán Maya archaeology for the uninitiated and as a source book on Maya archaeology for the specialist. It is a fine, solid contribution that will have lasting value.

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GRANT D. JONES. *Maya Resistance to Spanish Rule: Time and History on a Colonial Frontier*. Albuquerque: University of New Mexico Press. 1989. Pp. xiii, 365.

With the Spanish conquest of northern Yucatán, many Yucatec-speaking Maya fled southeastward. Refugee zones were created in areas comprising parts of modern-day Quintana Roo and Belize and into the Petén region of Guatemala. For the next one hundred fifty years, these exiles and their descendants sought to frustrate Spanish control through further retreat, rebellion, and passive resistance. They succeeded to a degree because the remote Maya frontier held little attraction for the Spaniards, who considered it uninhabitable and of little economic interest. Moreover, the conquerors lacked the resources necessary to mount major expeditions into the hinterlands. Yet the author emphasizes that these Indians of the southeast were not completely isolated and that they inevitably accepted some aspects of Spanish government and religion.

More important, Grant D. Jones maintains that the

Indians used the frontier to hide their ideas from the conquerors. "The underlying task of anthropological history," he writes, "must be to discover what ordinarily remained hidden from the European authors of our documents . . . to accomplish such discoveries of the Other in texts in which the cultural meaning of Maya behavior almost always eluded their authors" (p. 17). Notwithstanding the author's attempt to reconstruct the "temporal signification of Maya action," the "Other" remains elusive (p. 17). He does provide the reader with detailed information, however, based on little-known archival documents, to present a picture of early colonial life in southeastern Yucatán.

Jones chronicles the first Spanish penetration and settlement, the establishment of simple administration, the awarding of *encomiendas*, and early efforts at religious conversion. Eventually the Maya rebelled, with massacres of Spaniards in 1624 and 1638. The uprising was subdued, and the rebels were subjected to the same atrocities that they had practiced on the white men. Despite the efforts of the missionaries, the traditional Maya religion persisted, and, ultimately, many of the Spanish clergymen were martyred. Only with great difficulty were the conquerors able to establish some order through military action, and they still had not brought the Maya under their control by the late seventeenth century. This book covers the period to 1697, the year in which Spaniards finally sent a large expedition to conquer the Itza in the Petén. By then, however, the region of Belize had been well infested with English pirates-turned-logcutters, leading to the eventual colonization of the region by England.

Although the places studied in this work were sparsely settled by both Indians and Spaniards, remaining backwaters in the Spanish Indies, regional scholars will appreciate the fine details presented by the author. A useful appendix indicates locations of Maya settlements and provides good information on *encomiendas*.

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MARIETTA MORRISSEY. *Slave Women in the New World: Gender Stratification in the Caribbean*. (Studies in Historical Social Change.) Lawrence: University Press of Kansas. 1989. Pp. xiv, 202.

Unlike the works of U.S. historians, Caribbean historiography of the 1970s, and most of the 1980s, is silent on the wide-ranging and fascinating discourse on gender in the slave societies of plantation America. For some, this "refusal" to speak is rather surprising. During this period, perhaps more so than in any other, historians of the Caribbean have produced an impressive body of literature that is multidisciplinary in nature and conceived within explicitly stated theoretical frameworks. In 1989, however, the silence on

the topic of gender was broken. Three substantial books on the socioeconomic history of enslaved women appeared, each displaying a different theoretical perspective and methodology. Unfortunately for the reader, they seemed to have been researched and published in ignorance of each other, which may have softened somewhat their individual historiographical impact.

Of the three books, only Marietta Morrissey's can fairly be described as an extensive historiographical essay. But it is more than the usual unstimulating, but necessary, comprehensive stocktaking. It is a critical and intellectually provocative evaluation of over two hundred years of literature, designed and intended to promote the redressing of the conceptual weakness and empirical shallowness that appear when most interpretations of slave relations and society are viewed through the lens of gender.

Morrissey's principal complaint is that generations of historians, both male and female, refused to conduct specific and systematic research into the lives of women, the most oppressed and exploited slave group. As a result, they could not present the uniqueness and specificity of women's experiences but instead erected historiographic edifices that have no foundation in the concrete life experiences of nearly half of the region's inhabitants. Morrissey shows considerable control, however, in her judgment of even the greatest offenders and remains sympathetic to those historians who appear reluctant to embrace gender as an instrument of historical investigation.

The general contour of the book follows a clearly articulated Marxist feminism that also gives it an identifiable intellectual posture. In an energetically written theoretical survey, the author tells us that the book is based on a materialist perspective of history, which, in her opinion, offers the most penetrative methodological instruments for locating the interconnection of gender and patriarchy in the slave mode of production. By examining closely the literature on slave women's labor, domestic, and other relations, she provides many insights on the conceptual confusion that surround their biosocial world of fertility, fecundity, and reproduction.

Also in two carefully written chapters, Morrissey discusses the extensive demographic data that seek to explain slaves' inability to achieve positive growth. It is true, as she points out, that there is considerable uncertainty in the literature concerning slave women's social behavior and attitudes as mothers, laborers, mistresses, domestics, and rebels, but little in the way of suggestions for research is offered. Furthermore, her analysis of slave women's maturing and creolizing consciousness lacks depth and accounts in part for the too generalized interpretation of resistance. Yet these themes are critical to the discourse on the vibrant survivalist culture of slave women and the role it played in community formation.

A major strength of this book, however, is that it assesses comparatively the historiography of the Car-

ibbean across cultural and language lines. There is no English-language bias here. Even treatment is offered to the literature on the six major imperial groups of colonies. The reader is encouraged to see the many commonalities of slave women's lives that transcended consideration of imperial spatial dimensions. At the same time, the book's theoretical claims are weakened by Morrissey's dependence on the very literature that she criticizes, which suggests that perhaps conceptual departures are most persuasive when located in primary research rather than in reassessments of secondary work. But the book is clearly written with an impressive intellectual style and certainly constitutes required reading for all historians of the Caribbean, feminists or otherwise.

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GUY P. C. THOMSON. *Puebla de Los Angeles: Industry and Society in a Mexican City, 1700–1850*. (Dellplain Latin American Studies, number 25.) Boulder, Colo.: Westview. 1989. Pp. xxiv, 396. \$43.00.

Some things are worth waiting for. Guy P. C. Thomson's fine study of Puebla has been long in the making. The effort, care, and intelligence that characterize its pages are the result of extended consideration. Even in a historiography largely defined by superior regional studies, Thomson's entry is outstanding. Given its broad perspective, careful analysis, painstaking research, and nuanced understanding of "commerce and coalitions," this book is best compared to David Brading's study of León.

By the early seventeenth century, Puebla was an agricultural and woolen manufacturing center of first rank. But the prohibition of trade with Peru (1634) and the burgeoning competition of the Mexican Bajío left Puebla and its hinterlands a privileged backwater. Nevertheless, the recovery of the area's native population continued into the 1740s and provided a reservoir of labor that local merchants employed in an expanding cotton trade. Abetted by the Bourbon reforms, by the expansion of silver mining, and by the commercial backwash of the Napoleonic Wars, the boom in cottons continued into the early 1820s. But the colonial wars of the 1810s took their toll, and the collapse of silver mining severely reduced the supply of credit. Surging imports of British cottons after 1825 thus confronted an already weakened industry. Yet the "liberal" nostrum, a retreat into agriculture and comparative advantage, offered scant solace. Domestic markets for Puebla's surplus grains were limited by high transportation costs, and the lucrative Caribbean trade in wheat and wheat flour had already been seized by the United States. Although physically productive, the ability of Puebla's agriculture to absorb redundant labor from a shrinking industry was circumscribed. Hence, the city turned to tariff protection, import prohibitions, xenophobia, and political "conservatism" as answers to the gathering socioeconomic crisis.

nophobia, and political "conservatism" as answers to the gathering socioeconomic crisis.

I think Thomson's story is largely correct and would differ only with the tenor of the interpretation. In particular, the upshot of Puebla's often-complex and equivocal advocacy for protection was, as Stephen Haber's current work shows, a cotton industry remarkable only for its relative inefficiency. Although the costs of chronic unemployment and its attendant political problems might well be greater than the allocative inefficiencies that tariffs and prohibitions entail, Thomson's rehearsing of the special pleading of Puebla's entrepreneurs hardly makes for a convincing argument. I also think Thomson goes somewhat awry on the question of the balance of payments. Mexico could afford to trade silver for imports without much difficulty as long as its mines were productive. But the total remission of silver to Britain, France, and the United States consumed the bulk of the country's drastically reduced production until well into the 1840s. Small wonder that monetary disturbances and rising interest rates were a plague.

The high caliber of recent British scholarship on Mexico is its own best argument for free trade. How ironic that the greatest beneficiaries of an idea are sometimes least prepared to accept it.

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THOMAS BENJAMIN. *A Rich Land, a Poor People: Politics and Society in Modern Chiapas*. Albuquerque: University of New Mexico Press. 1989. Pp. xxii, 350. \$40.00.

This finely crafted, eminently readable history of modern Chiapas makes an important contribution to our understanding of elite-driven politics in regional Mexico. Thomas Benjamin's research of political correspondence, administrative reports, newspapers, and broadsides housed in repositories in Tuxtla Gutiérrez, Mexico City, and Washington, D.C., is exhaustive, and it is matched by his mastery of the secondary literature on Chiapas and modern Mexico. Here, as in his earlier essays on regional Porfirian and revolutionary-era historiography, Benjamin demonstrates that few historians of Mexico—particularly on this side of the border—can rival his appreciation of *historia patria*. Benjamin mines the local *crónica* for nuggets that lend color and substance to his own political narrative, and, in the absence of collected oral traditions, the genre allows him to reconstruct elite networks, projects, and self-perceptions.

As the volume's title suggests, the author traces the Chiapanecan elite's progress at the expense of the masses in the post-independence era, giving particular emphasis to the period from 1890 to 1950. Benjamin maintains that change always percolated within a broader continuity. In spite of short-term factional conflicts within the elite "Chiapanecan family"—Cristobalense versus Tuxteco, central highland "tradi-

tionalists" versus central valley "modernizers," states' righters versus nationalists—the regional elite never lost its strategic capacity to close ranks and fend off challenges to its hegemony from without and below. In the process, the "familia chiapaneca" survived the Reforma, efficiently transformed the Mexican revolution of 1910 into its own counterrevolution, and has (thus far, at least) managed to defeat, coopt, or survive a series of more ideologically radical postrevolutionary mobilizations. Benjamin argues compellingly that, throughout its brutal history, Chiapas has remained "distant, provincial, and underdeveloped" (p. xiii)—one of Mexico's two or three poorest states—largely because "the government and the finqueros are the same thing" (p. 223). Even today, the prevailing image of the state among other Mexicans is that of a medieval castle surrounded by a hostile jungle.

Although Benjamin intended to write the history of Chiapas "in its own right" (p. xvi), his volume also integrates national-level events with local happenings as successfully as any Mexican regional history to date. His achievement is even more impressive given the study's broad sweep (1821 to the present).

Nevertheless, some reservations are in order. In revising the fine dissertation that lies at the core of the present work, Benjamin has excessively pruned social analysis and relevant theory in order to showcase his detailed political narrative. Despite the promise of its subtitle, this is a volume that tells us much more about Chiapanecan politics than society. Benjamin is on firm ground when he chronicles the socioeconomic impact of government, but he never provides us with anything approaching a *radiografía* of the social bases of politics.

Clearly, there is no book that can accomplish everything, much less a first book. Given the inevitable limitations of time, money, and energy, Benjamin's strategy emphasizes institutional political sources over such staples of social research as oral history and judicial records. Moreover, he was obliged to write his monograph before several long-term investigations into Chiapanecan social and agrarian history bore their first fruits (for example, Daniela Spenser and Friedl Baumann on Soconusco and Jan Rus on Chamula).

Consequently, his analyses of the origins and character of such episodes as the "states' rights" movement of the 1910s, the socialist mobilization of the 1920s, and the campesino movement of the 1980s often raise more questions than they answer. Chiapas's history, brutal as it is, too often is reduced by Benjamin to what the ruling elite did to the Indian and campesino masses. In the process, we gain little appreciation for how the campesinos managed the responses they did, some of which (particularly in the 1920s and 1980s) represented formidable moments of protest. An adequate history of elite power and

interests also entails a reconstruction of popular protest and accommodation.

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V. W. LEONARD. *Politicians, Pupils, and Priests: Argentine Education since 1943*. (American University Studies, Series 22; Latin American Studies, number 2.) New York: Peter Lang. 1989. Pp. xiv, 456.

This work provides a survey of the relationship between Argentine governments and the Catholic church within the specific context of the education of children, a subject rarely taken up by historians of Argentina; V. W. Leonard's book is the only study in English that I know about. Furthermore, the entire area of church-state relations is relatively absent from the list of mined topics on Argentina, a region in which the Catholic church remained historically modest in its material and political assets compared to its holdings in states such as Mexico and Peru. The analysis of Argentina's church concentrates on three historical moments: its association with the Rosas dictatorship (1829–52); its relationship with Domingo Perón in the 1940s and 1950s; and its (somewhat) divided attitude of toleration for human rights violations during the last military regime (1976–83).

The military government's domestic policies concerning education expressed some of the shared aspirations of the conservative sectors among civilians and the military. High schools and especially universities were considered centers of political dissent and leftist ideology that served to undermine the social and political order. Thus, one of the government's goals was to depoliticize centers of learning. In the end, it did not work, of course, but the process led to one of the country's most thorough purges of intellectuals and a profound sense of intimidation within the universities and normal and secondary schools.

Leonard opens her work with two chapters covering the late colonial and postindependence eras up to the late nineteenth century in a very brief survey of the role, function, and administrative structures of the educational system and of the church's fundamental role. Liberal secular authorities enacted legislation that effectively eliminated the church from awarding and legitimating graduation degrees from secondary schools (Law 934 passed in 1877). The book begins in earnest with Juan Perón's rise to a position of authority and concludes with the last stages of Raúl Alfonsín's administration in 1988.

Leonard concentrates on the two-part objective of the Catholic church: first, determination of the church's own curricula and educational administration, not only free from federal regulation but also in ideological partnership with the state; and second, the enhancement of the church's influence on the federal and provincial authorities in order to require



non-Catholic and public schools to adhere to curricular elements that reflected the church's views on morality training and the political obligations of the citizenry. In what Leonard describes as a "holy alliance" that lasted from 1943 to 1954, the church and Perón commonly pushed for and succeeded in enacting legislation that required religious education in public schools, that subsidized the salaries of private school teachers, including church-owned institutions, and that, in general, served to boost the prestige, financial support, and ideological stance of the church in matters of education. Subsequent cleavages in church-state cooperation under Perón revolved around issues of welfare and did not alter the consensus on educational policies. As Leonard nears her conclusions, she relies on statistics related to school attendance, dropout rates, university degrees, and other data from domestic and international organiza-

tions. Not surprisingly, she draws a picture of a longitudinal decline in educational achievements.

The work follows a chronological style that often skims the surface of complex issues in the interest of covering the whole period from 1943 until the 1980s, resulting in an uneven thematic coverage that leaves the reader wishing for more detailed analysis of some of the fundamental ideologies driving the debates on educational policies. Nonetheless, an important subject is addressed in this work. Argentina's economic decline, political difficulties, and ideological schisms can be more fully understood from within the context of the country's educational conduct, limited budgets in the area of secondary and university training, and the associated relationship between brain drain and national development.

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## Collected Essays

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These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed.

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## Communications

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### FILM REVIEWS

#### TO THE EDITOR:

John Mraz has written an analysis of *Azul* [AHR, 95 (October 1990): 1139–41] that is so wrongheaded I find myself compelled to respond.

First, it is absurd to class my film as a work of Direct Cinema. Simply because a work declines to employ voice-over narration does not make it direct cinema. For example, there is a great deemphasis if not absolute elimination of the active interview in most important direct cinema works; *Azul* relies heavily on the active interview to weave perspective into the poetic narrative. What work of direct cinema has ever included the director's choreographed recital of a poem?

Mraz's lack of familiarity with the medium is not merely formal. Ultimately, direct cinema's purpose is to extract the perceptions of history out of a series of unedited moments reflecting reality. My approach to the historical essence of a film has been quite the opposite: rather than extract truth from the unexpurgated moment, I have chosen to structure a nation's history as a narrative document of its spirit. *Azul* was very consciously constructed (again, not a method of direct cinema) to follow the undulations of poetry, to move back and forth through history, not driven by a clock, rather by thematic development.

Mraz's knowledge of poetry is also suspect. To confuse Rubén Darío's poetic elitism with an apolitical stance is a syntactical sleight of hand that cannot be allowed to go unchallenged. One need only read Darío's poems: *Paz* (1917), *Canto a la Unión Centroamericana* (1889), or the famous *Oda a Roosevelt* (1904) to see that Darío was anything but apolitical in his work. Nor did he choose to lead the life of a

reclusive "ivory tower" poet. His outspoken work as a journalist and Nicaraguan diplomat was a paradigmatic example of the Latin American man of letters and action. Saying Darío is apolitical is like saying the *Divine Comedy* is solely an interpretation of Scripture.

More to the point is Mraz's sense of confusion when confronted by José Coronel Urtecho's statement "without Darío there could be no Sandino." Why is this such a curious statement? Two lines later, Urtecho says, "Nor could Darío have existed without Bolívar or Martí." Darío, like James Joyce, came to be the great defining voice of his nation's conscience. Simón Bolívar and José Martí inspired Darío, helped to evolve his image of Nicaragua's nationhood at a time when all Latin America was struggling to reshape itself. Darío then gave voice, shape, color, and passion to this new country's struggle for self. In all of Augusto César Sandino's writings, spare as they are, one can see the influence of Darío. Sandino's army—ragtag, ill-equipped, impoverished, illiterate—fought for its country's independence, not for abstract principle. They fought for their land and their homes, and the poetry in their leader's words constantly reflected what was in their hearts. This is not romantic myth making: read the texts of Sandino's speeches, read the testimony of his soldiers, look at the soldiers who were in my film, the grandsons and great-grandsons of Sandino's soldiers. Do they quote Marx or do they quote poets as their source of strength and inspiration?

Next, Mraz, having bumbled through cinematic and poetic analysis, falls flat on his face when it comes to simple scholarship. He laments the fact that a scene used to illustrate the death of Leonel Rugama was not the actual footage of the brutal attack by Anastasio Somoza's National Guard on the lone defender. This was not my attempt to "slip one by" the audience, as evidenced by the fact that I used shots with newsreel "chyron" dates included. But quick research would reveal a well-known fact that Somoza destroyed almost all Nicaraguan TV footage by bombing the television station before escaping Managua. Clearly, the actual footage would have been preferable but not for the reasons Mraz states. *Azul* is not an analysis of the uses of mass media during the Somoza dictatorship. It is precisely an attempt to create something

antithetical to quick-fix mass media. Further, the myth that archival footage is better suited to presenting the truth is just that—a myth. Archival footage has often been manipulated to meet the ideological needs of a particular documentary.

Mraz at least figures out that the inclusion of scenes from *Walker* was meant as a critique of Hollywood's incessant rape of history. [*Walker* is reviewed in *AHR*, 94 (October 1989): 1047–49.] Indeed, the making of *Walker*, even though the filmmaker professed a deep affinity with the revolution, was essentially a re-invasion of Nicaragua by “Cox’s army,” and, while paying lip service to politically “correct” positions, *Walker* is nothing more than another gringo-centric view of history with Nicaraguans functioning as backdrop and cannon fodder. Comparing my film to *Walker* is an infuriating insult. Director Alex Cox’s ahistorical sin is British and North American arrogance about the source and motion of history. Mraz’s historical misperception is to interpret reality and integrity in much the same way as Cox. One of the reasons to make *Azul* was to try and undo this ever-present view of Latin American history as originating in North America.

Mraz also reveals himself as a reviewer unable to distinguish subtlety, nuance, and irony. Of course the Sandinistas have not eradicated sexism; far from it. The prisoners’ comments were not meant to prove this. It was clear that both the prisoner and his critic had been fed the “correct line” by the guards and were trying to perform according to the “script” given them.

Are poets truly in power? Am I guilty of myth making again? Every single member of the Sandinista directorate writes poetry except Sergio Ramirez, who is a novelist. Many members of the current Chamorro government are also poets. How many members of President George Bush’s Cabinet can even write grammatically correct English? Dan Quayle?

Mraz faults the film for not offering enough Sandinista self-criticism of their revolution. *Azul* sought to reflect the commonly perceived reality that the debate fell far short of the young government’s needs to find ways to implement change. Michelle Najlis states this quite concisely during her interview. Witness the Sandinista loss in the January 1990 elections; their subsequent analysis of “what went wrong” often cites the lack of constant, aggressive self-criticism as the major internal flaw in their revolutionary process. In choosing to let the Nicaraguans speak for themselves, one is obliged to listen to what they say and also to what they do not say. I am disappointed that Mraz is unschooled in interpreting the value of silence both in history and filmmaking.

Mraz also faults me for using memories as the equivalent of history, personal perspectives for reality, and for short-changing the critics of the revolution because I was aiming the film at a U.S. audience. What this amounts to is a statement that the film is a

conscious effort to distort the truth. If this was the Old West, I’d reach for my gun.

What is history if not memory—often faulty, incomplete memory? What is the understanding of history if it is not accompanied by perspective and context? Careful research is the great gift of the historian to our understanding of the human condition. From that painstaking work, there emerges a series of items that we, for lack of a better term, call facts. But facts are the products of a very inexact science. A fact only becomes a part of the truth if one can interpret it: Mraz’s search for the historical truth in *Azul*, I am sad to say, was misdirected by his failure to distinguish fact from truth. Without interpretation, facts fall silently in the forest like dead trees. It is true they have fallen, but no one has heard them fall. *Azul* was my flawed attempt to find a path into that forest. A path positing a more active role for poetry in shaping history.

ROLAND LEGIARDI-LAURA  
New York City

#### JOHN MRAZ REPLIES:

Being hit by “friendly fire” is a most disconcerting experience. That, at any rate, is the feeling I was left with after reading Roland Legiardi-Laura’s response to my review of *Azul*. Let me say at the outset that I like *Azul*: I think it is interesting and informative. As I stated in my review, “[A] lovely, lyrical documentary film, [*Azul*] incorporates poetry into history, opening up alternative ways of thinking about historical representation and counteracting the propaganda of U.S. politicians who maintain that Nicaragua constitutes a ‘threat’ to democracy in the Western hemisphere.” To me, that seemed a significant accomplishment for a low-budget, independently produced documentary film, but it was apparently not sufficient for Legiardi-Laura, whose measured reaction exposed some exaggerated pretensions.

While I find myself surprised by an attack from someone with whom I assumed shared political sympathies, I sense that Legiardi-Laura has found himself out of his element in the developing debate on historical cinema. He and his highly personal work have stumbled into a crossfire between some historians who are beginning to define the specific contributions of film, video, and photography to the discipline of history and others who feel that even to open such a question demonstrates “bias against scholarship” (see Thomas Sowell’s letter in the *AHR*, 96 [February 1991]: 328). Nonetheless, Legiardi-Laura’s film and letter give us the opportunity to explore issues important to this emerging discussion.

As near as I can determine from his confused diatribe, Legiardi-Laura believes that history is memory. This is certainly apparent in the film, whose narrative is largely constructed through interviews. Scholars who utilize interviews in their research know that their role as *historians* is to get beyond and behind

what they are told. Interviews must be contextualized in order to provoke a critical response to them and, hopefully, to the work as a whole. What is missing from *Azul* is the formal resonance that could create such a critical ambience. This requires subtle and complex skills; that such an endeavor is beyond Legiardi-Laura's capacities is evident by comparing his filmic treatment of the interview with Coronel Urtecho to that of the prisoners. There is nothing whatever in the cinematic structures of these interviews to indicate that viewers are to perceive Urtecho's comments as truth incarnate but assume a critical distance from the prisoners' statements.

In the better historical documentaries, interviews are juxtaposed with other filmic elements such as newsreel footage, still photo montage, or, at the very least, contradictory interviews. Through such juxtaposition, the speaker's point of view is placed within a larger context; the objective aura that surrounds the "talking head" is subjectivized and made relative to the competing perspectives. The "truth" of such films arises out of the conflicts between the various elements, and, in the best of cases, the director's own subjectivity is itself alluded to. Unfortunately, this formal richness and complexity is absent in *Azul*, and, rather than listening to criticism to find out how the film might look to others, Legiardi-Laura rigidly insists on what he *thinks* he has accomplished.

Legiardi-Laura's use and defense of anachronistic footage is yet another shortcoming. It may well be that no footage of the Rugama incident exists—although anyone who has worked much in the largely improvised and often uncatalogued archives of Latin America would be hesitant to talk (as does Legiardi-Laura) about "quick research" revealing "well-known facts." However, the question remains: what did the director *choose* to do in the absence of such footage, and what is the meaning of that choice? To *choose* to show "cut-aways" of street fighting filmed nine years after Rugama's murder clearly equates the two events, even if the anachronistic footage includes newsreel chyron dates. Using images as general and abstract illustrations for an unrelated text is the easy way out, one that has given visual history the bad reputation it has so often richly deserved.

As historian-cinéastes, we must go beyond illustrationism, utilizing the particularity of images to lead us ever deeper into the concrete historical situations we are studying. We have to develop alternatives to the expedience of "illustrational" images, whether that means being diligent in searching out photos from newspaper and private archives or being creative enough to use, for example, a black screen with a written text such as "No TV footage of this incident exists because Somoza bombed the station." That archival footage and photos have "often been manipulated" is obvious; what is equally apparent is that the visual historian's job is to insist on alternatives to such manipulation, either through criticism or through example.

Aside from the preceding observations on historical cinema, it is important to address some of the specific points made by Legiardi-Laura. First, I am unclear about the reasons for Legiardi-Laura's violent reaction to my description of *Azul* as "direct cinema." Any film that relies so heavily on interviews and "participant narration" seemed most easily encapsulated in that term. Second, it is clear that Legiardi-Laura and I see Rubén Darío through different optics. Darío was and is many things to many people. It may be useful to mention that my opinions about his work have been strongly influenced by Jean Franco, a leading expert in this field who established a trenchant contrast between Darío's symbolic rebellion (and artistic elitism) and José Martí's commitment to political and aesthetic revolution. (See *The Modern Culture of Latin America* [1967], 21–25.) Third, Legiardi-Laura's ingenuousness is as apparent in relation to Darío as it is in his references to the Sandinista directorate as "poets in power." Not all those who think they write poetry are poets, but it is worth noting that Nicaragua was a rare case in which a nation beleaguered by the U.S. military machine had the opportunity to be governed by cultured individuals. Fourth, as to Legiardi-Laura's treatment of the lack of Sandinista self-criticism, I am left with a question: as a filmmaker, does one represent a reality by simply reproducing it, or are there more effective cinematic strategies for articulating that perspective?

Finally, at the risk of further inciting Legiardi-Laura's *pistolero* complex, let me say that I am perhaps most confused by his reaction to my impression that the film was made for U.S. audiences. I can only imagine that I have touched something far deeper than was immediately apparent. It seemed unproblematic to assume that a film subtitled in English and made by someone living in New York was probably directed toward a U.S. audience. Further, as with many of Legiardi-Laura's responses, I find it difficult to understand where the "beef" is. The United States is where the battle for and against Sandinismo has been waged in a real sense; without massive U.S. aid to the *contra* mercenaries, there would have been no war. If *Azul* was not made to counteract the propaganda of U.S. politicians and mass media, why was it made?

In the end, it is impossible for me not to feel suspicious of where Legiardi-Laura's interests lie. Too many "politically correct" artists and intellectuals who live in the United States use Latin American revolutions to further their own careers. This sort of "internal colonialism" is well known to those of us who choose to live in Latin America and to whom the most immediate issue is the survival of progressive regimes capable of transforming people's lives. Evidently, Legiardi-Laura has other priorities.

JOHN MRAZ

Universidad Autónoma de Puebla



## TO THE EDITOR:

This is in response to the letter of Thomas Sowell to you on movie reviews [*AHR*, 96 (February 1991): 328]. I think I can understand why some people who have devoted their lives to scholarship (I assume Sowell is a scholar) primarily through the print medium might be skeptical about devoting space on the printed page to reviews of films. There is said to be a problem nowadays that people read less and spend more time in front of a screen (blue or silver); and no doubt there is—though when I was a child the same lament was in the air. But, since our culture is becoming more and more visual, we ought to face this fact intelligently and creatively—not by dismissing movie reviewing as somehow a “bias against scholarship” (I quote Sowell); and not by giving up books, either. Most readers of the *AHR* are teachers and professors who are constantly looking for ways to freshen up their scholarship and their teaching. Perusing articles and book reviews is one way to do this. But so is studying and using films.

If I read things rightly, historians are increasingly concerned about how to integrate “culture” into their study of history—just as in recent decades some have tried to incorporate social, ethnic, or gender issues into it. Since cinema is an increasingly crucial ingredient of cultural studies, I applaud the *AHR* and Robert Rosenstone for launching this new enterprise. If there were to be a deluge of mail protesting such reviews as Sowell seems to suggest must be happening, perhaps the editors and other interested parties should rethink the matter. About the quality of the reviews, I cannot say much. Sowell uses the terms “pretentious obscurantism” and “pomposity” to describe them. That, I suppose, is a matter of opinion. My opinion is: let scholars of many persuasions and styles take up the critical task and let us all try to find out how cultural artifacts actually relate to the world that surrounds and generates them.

RICHARD STITES  
*Georgetown University*

## REVIEWS OF BOOKS

## TO THE EDITOR:

In my opinion, Lawrence D. Walker's review of Carole Fink's book, *Marc Bloch: A Life in History* (*AHR*,

96 [February 1991]: 128) did not do justice to this richly documented, elegantly written study of the celebrated co-founder of the *Annales*. The review struck me as very narrowly focused and idiosyncratic, revealing a great deal about the reviewer's own appreciation of Bloch and his work but very little about the contents of Fink's work.

A reviewer is certainly entitled, nay, duty-bound, to challenge untenable interpretations or expose factual errors in a book under review. Walker does neither. Instead, he engages in a discursive rumination about the appropriate way to write intellectual history and finds Fink's work deficient in that regard. I found particularly puzzling his last paragraph, which begins: “Bloch's life would be of interest had he not been a genius at innovation. But he was” (p. 128). Ergo, the details of Bloch's personal and professional life—which Fink re-creates on the basis of exhaustive research in primary sources and interviews—are matters of minor interest that divert attention from “the creativity of the man and the movement”? Other references to Bloch's “creativity” and “imagination” in the review reinforce the impression that Walker would have settled for nothing less than an exercise in hagiography. In my view, Fink's decision to treat her subject as a human being rather than an icon is cause for approbation, not disappointment.

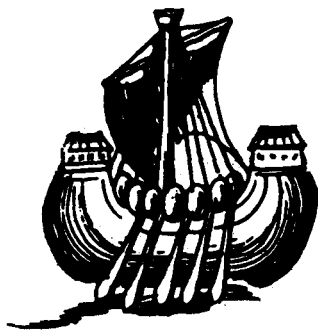
WILLIAM R. KEYLOR  
*Boston University*

## LAWRENCE WALKER REPLIES:

In no way did I intend to detract from Carole Fink's achievement in producing *Marc Bloch: A Life in History*. The book is everything Keylor says it is. I differ with him in his estimation of the task of the reviewer, which is not simply to “challenge untenable interpretations” and “expose factual errors” but to assess the overall range and scope of a book and place it in the ongoing stream of research. I hoped to do that. As for Bloch's “creativity” and “imagination,” I cannot believe, had he not possessed those very qualities, that Carole Fink would have written the biography about which Keylor and I now argue.

LAWRENCE D. WALKER  
*University of Utah*

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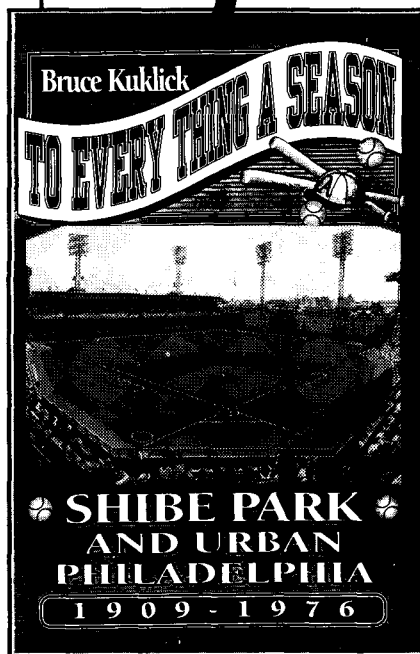
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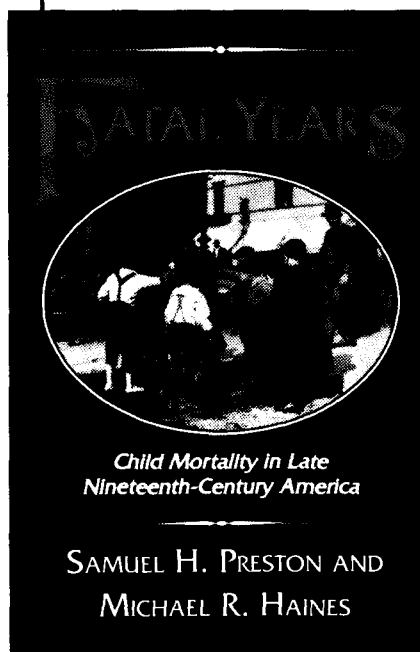


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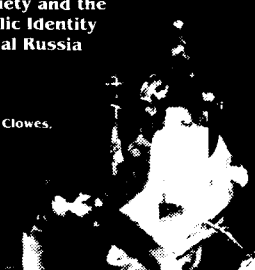
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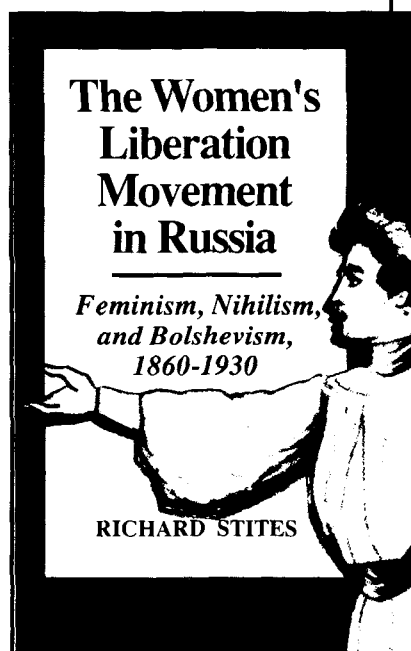
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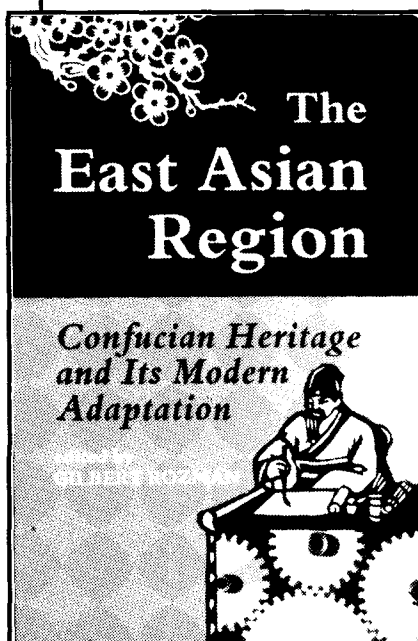
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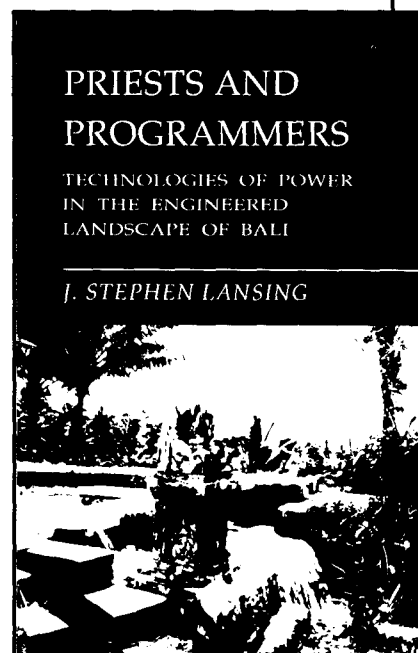
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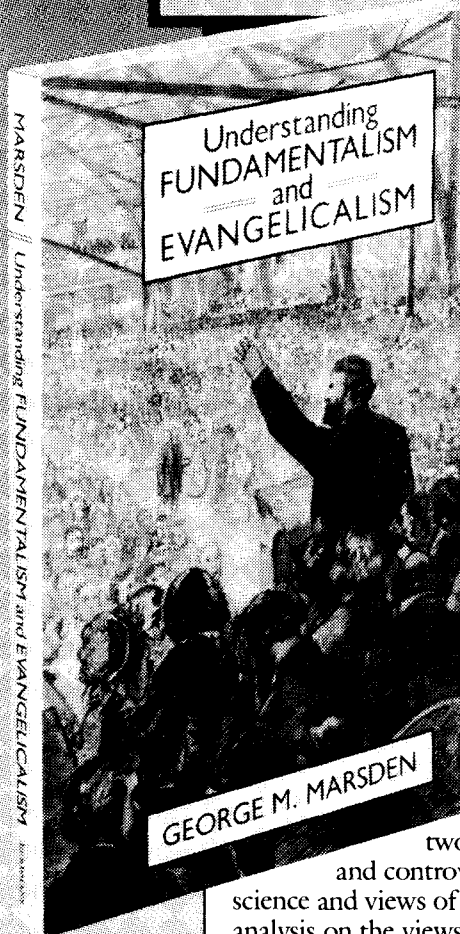
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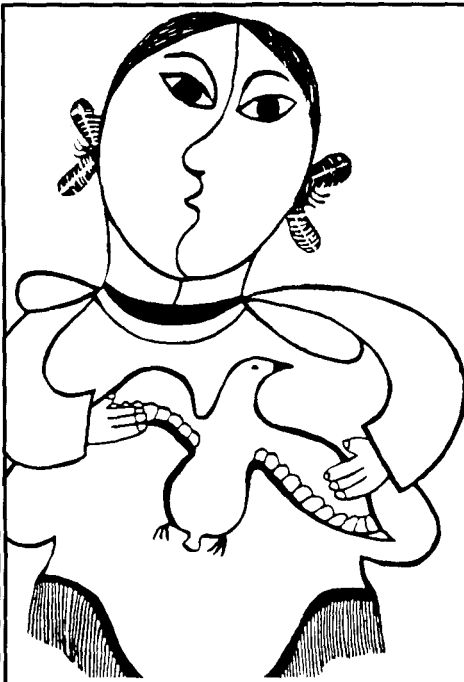
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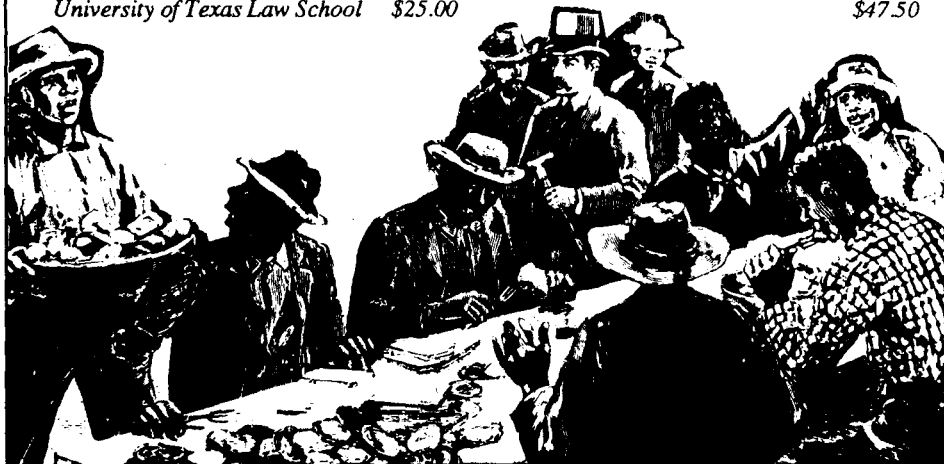
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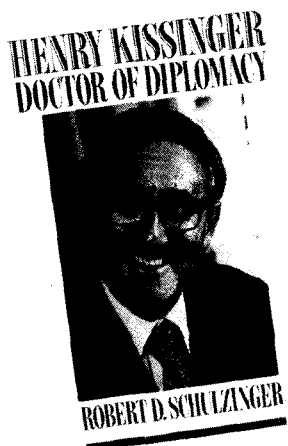
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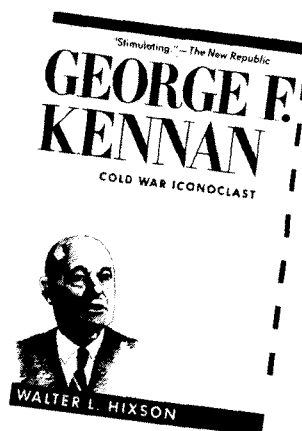
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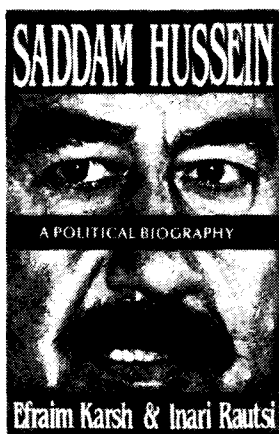
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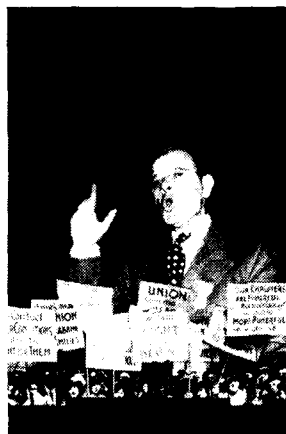
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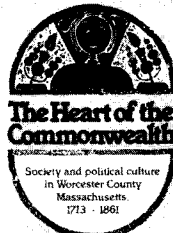
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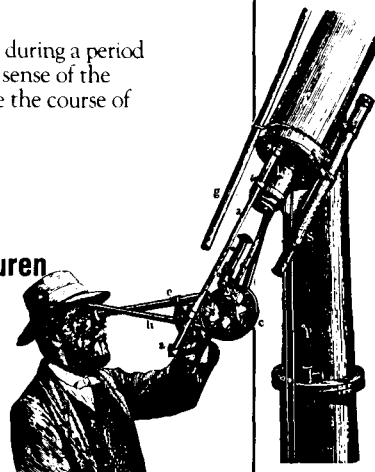
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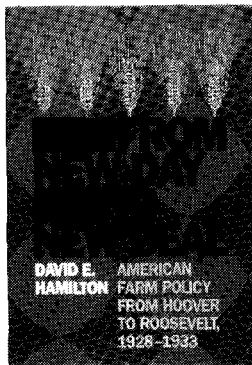
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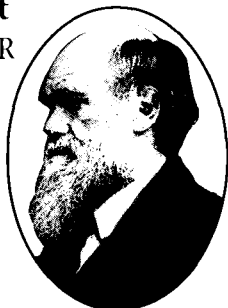
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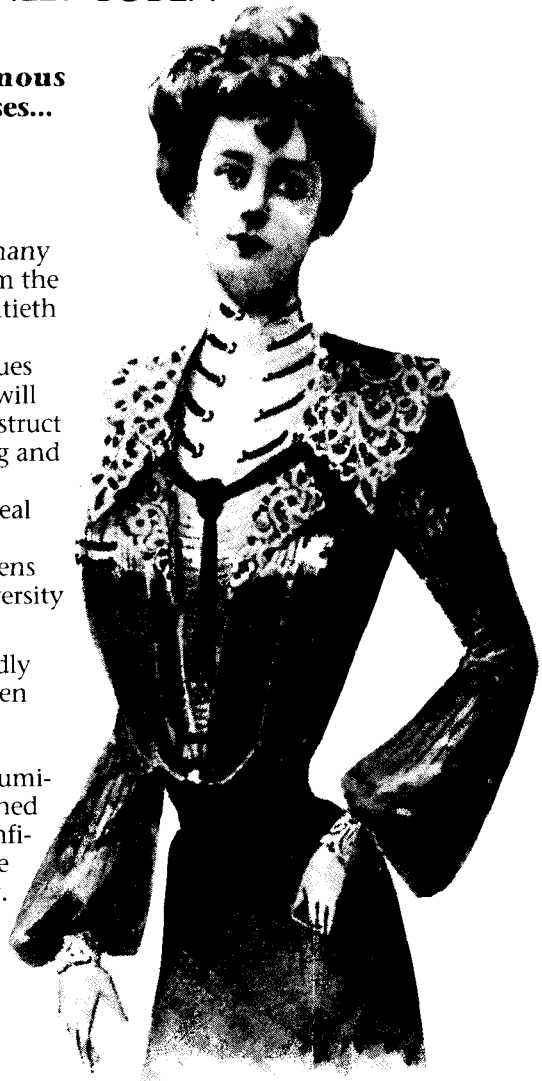
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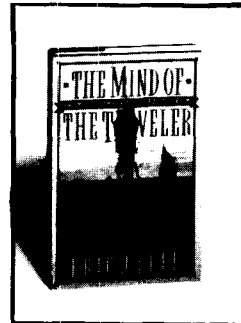
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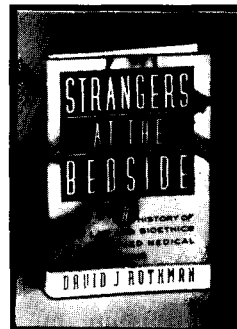
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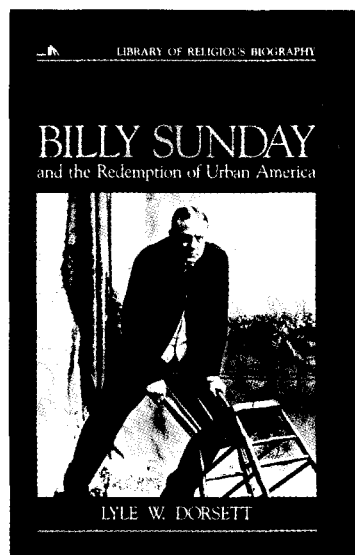
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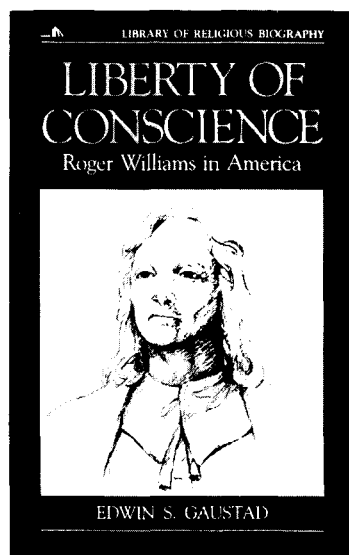
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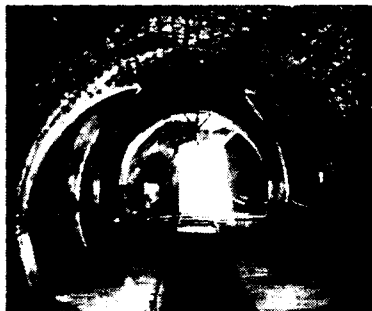
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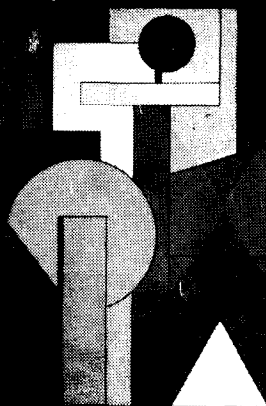
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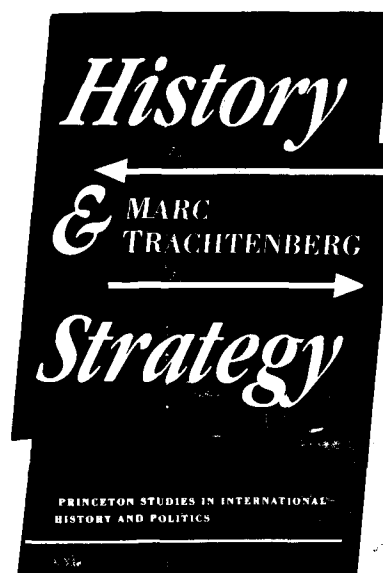
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